

Psychological Bulletin

RECENT DEVELOPMENTS IN THE FIELD OF EMOTION

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This paper attempts a critical survey of the work in the field of emotion during the last few years. A good summary of the earlier work in the field may be found in Ruckmick's *Psychology of feeling and emotion* (78). Lund's recent *Emotions: their psychological, physiological, and educative implications* supplements Ruckmick, particularly in the direction of physiology and metabolism (67). An excellent survey of the experimental work is given in Landis's chapter in *A handbook of general experimental psychology* (62), and a brief treatment of some of the experimental approaches in Woodworth's *Experimental psychology* (92). There will be no attempt to present a complete bibliography here. Mention will be made only of those studies which seem to indicate significant trends in the field. For a complementary treatment of the field of affectivity, the reader is referred to a previous *Bulletin* article by the present author (52).

Developments in the field of emotion have paralleled closely those in other branches of psychology. There has been the same initial use of naïve concepts and elementary experimental procedures, accompanied by a blind reliance on an oversimplified and unreal physiology. The subsequent failure of this approach brought with it a temporary disillusionment and skepticism which have in turn been dispersed as newer and more suitable concepts have been introduced along with more appropriate and adequate techniques for their investigation.

In particular, the psychology of emotion has gained by the current trend toward relativism and away from the search for simple absolutes. We are no longer so apt to think of emotion as a "thing" *sui generis*, unique and clearly separable from other psychological phenomena. Instead of seeking only for unique identities, we are

increasingly aware of the importance of a relative statement of the manifold conditions effective at any moment and the resulting changes in selected aspects of behavior.

The revolt against absolutism has led Duffy to deny that the concept of *emotion* serves any useful purpose in psychology (21). She points out that the study of emotion *sui generis* has led to naught and suggests abandoning the term and concentrating attention upon "the variations in certain fundamental *aspects* or *dimensions* of behavior in general as these occur under varying stimulation and as they correlate with each other" (p. 197). It is a similar point of view which has led Skinner to rebel against the concept of emotion as a series of unique responses and to substitute the concept of emotion as a change in the strength of reflex response. "The important thing is the recognition of a change in strength as a primary datum and the determination of the functional relationship between the strength and some operation" (84, p. 409).

While most psychologists today would applaud Duffy's relativism, few feel the need of discarding the concept of emotion. It serves as a term of convenience to map off a rough, inexactely defined territory for study within the larger field of behavior in general. Its definition would run something like this: In certain emergency situations of biological importance to the organism, usually involving the blocking of some vital drive or some threat to the well-being of the organism, there is an interruption of the current behavior with the appearance of innate responses of a patterned nature. These responses are particularly noticeable in the viscera, but also contain peripheral elements, and may be accompanied by specific conscious contents. The teleological assumption is often made that these responses are aimed at the successful resolution of the biological emergency.

This "definition" is not empirically established. It is not operational. It represents a rough statement of the implicit opinion, the guiding formulation which has supplied the orienting framework for the thinking of those who have worked in the field. It is a blend of fact, of theory, and even of hope, and partakes of the nature of a statement of faith. One cannot help but object to it as a scientific definition, but one must admit that such broad, often unconsidered conceptions offer the motivating background for the more specific, exact investigations of science proper. To attempt to delimit this definition further, to give a more exact meaning to its component

statements, would be to misunderstand its purpose and would necessitate anticipating the results of experimental work which is being carried on today or even that which must be carried on in the future.

Nevertheless, nebulous concept though it may be, emotion has served to focus attention upon one field of psychology, a field that has historical continuity, present-day functional significance, and one that, however lax it may have been in producing unique and individual data, has certainly added greatly to our knowledge of consciousness, of the physiological and neurological substrata of behavior, and of reflex response, as well as to our understanding of more molar behavioral concepts such as frustration, adaptation, etc. The above definition offers an organization which the article will follow by dealing with recent contributions under the heading of stimulus conditions, visceral behavior, peripheral behavior, conscious content, and teleological interpretation.

STIMULUS CONDITIONS

Very little attention has been paid to stimulus conditions as such. Mørey (70) has recently stressed this point. Most experimenters have concentrated on the response side of the picture, and there is none of the detailed study of stimuli which one finds in affectivity. Detailed description, analysis, classification, and systematic variation are seldom attempted with emotional stimuli. Even such rough beginnings as the demonstration by Landis and Hunt (64) that the startle pattern could be elicited by stimuli presented to different modalities (vision, audition, skin senses, etc.) are rare, although Winslow recently has reported a test of stimulus equivalence in connection with a peculiar emotional response in a single monkey (91).

In part this neglect is due to the difficulty of establishing emotional value. Emotion at present is registered in the laboratory through various bodily responses whose use as indicators involves complicated apparatus and laborious computations. One cannot imagine a psychologist with patience enough to make a careful, systematic study of the emotional value of stimuli of the same sort that Guilford (41) has made for the relation of hedonic value to the various aspects of color.

Our increasing relativism also has taken our interest away from the stimulus *per se*. As Dennis (19) has pointed out recently in discussing the infant's reaction to restraint, the Watsonian days

of listing specific stimuli for specific emotional responses are over. Infant behavior beyond the first few months is not produced by absolute stimuli, *i.e.* loud noises, loss of support, restraint of movement, but by "relative stimuli such as strange objects and frustrating situations. What is strange and what is frustrating necessarily depends upon the stimulation-history of the child" (p. 14).

In general this relativism has been healthy, and the Gestalt stress upon the "psychological field" has been a stimulating antidote to the oversimplicity of the stimulus-response formula. However, while the field theory does stress the complexity of the conditions responsible for behavior at any moment, it does not deny the possibility, even the desirability, of some sort of analysis of these conditions. While it tends to result in the use of molar concepts such as frustration, novelty, surprise, rather than more molecular ones such as restraint, loss of support, sudden loud noise, there is still a place for the intelligent study of such molecular aspects of the stimulus field. There is no need for the complete abandonment of the idea of absolute stimuli.

From the experimental side more attention to stimulus conditions certainly would be desirable in standardizing procedures and rendering results transposable from one study to another. No wonder that the literature on the emotional response to pain shows conflicting results, when a pain stimulus in one study is defined as a pin prick and in another as the slow tightening of a thumb screw until the subject no longer can endure the torture involved. Something like the rough standardization provided by the use of a .22 caliber blank cartridge in studying startle would be an improvement.

VISCERAL RESPONSE

The term "visceral" is used here to refer to internal responses connected with vital metabolic processes largely effected through smooth muscle and the autonomic nervous system. We shall mention breathing, cardiac action, and the galvanic skin response as typical.

Previous experimental work with these phenomena has failed to reveal exact patterns of response. There are two qualifications to this "failure," however. In the first place, our techniques of observation have been too simple. In the second place, we may have been too demanding in our interpretation of the exactness and specificity within which scientific prediction has to operate to be

valuable (22). Our study of emotion may not have yielded exact data like those for such functions as brightness discrimination (although these in turn seem less exact to specialists in the field), but we have amassed much general material which is useful psychiatrically in the alleviation of cardiac and digestive disorders in emotional states and in word association, crime detection, etc. As yet no psychologist can write an exact equation for the visceral responses in emotion, but every psychologist knows he will ruin his dinner if he loses his temper beforehand.

During the last few years there have been two noticeable trends in the study of visceral responses in emotion. One has been the increasing realization of the complexity of the responses, with an attendant increase in the use of sophisticated analytic techniques in their study. The other has been the increasingly important role assigned to the parasympathetic branch of the autonomic nervous system. We are realizing that emotion is an affair of the entire autonomic system and not merely the sympathetic branch (57).

Landis and Hunt (64) have pointed out the difficulty of isolating primary visceral elements of the startle response. The startle pattern involves a strong general flexion response which in itself is a stimulus to visceral activity. Since this muscular response begins within .05 second following the stimulus, it is difficult to tell whether breathing and cardiac irregularities are primary elements of the startle response or the secondary results of the muscular movement involved. Some type of fine temporal analysis like that used in the photographic study of the overt startle pattern is necessary here, but unfortunately the muscular movement in startle interferes mechanically with the recording in most of our present means of measuring visceral response.

Indeed, when one considers the complex interrelationships between the various viscera, effected both neurally and humorally, one wonders that the picture is as clear as it is. The heart rate varies with posture and shows a rhythmic variation with breathing (7, 90). Gellhorn, Darrow, and Yesinick have recently demonstrated that an increase in blood pressure automatically produces parasympathetic excitation and sympathetic inhibition (35). With the specific innervation of any visceral organ arriving against a background of tonic excitation already being furnished by both branches of the autonomic nervous system, with this specific innervation potentially arousing any number of automatic balance functions, and with the organ also subject to further concomitant

innervation resulting from the response picture in other parts of the organism, the need for a complex statement of the possibilities of response in terms of a multiple of stimulus conditions becomes evident.

Assuming that sympathetic and parasympathetic effects reinforce one another in producing the galvanic skin response but oppose one another in effecting blood pressure changes, Darling and Darrow (13) have proposed a two-dimensional "autonomogram" for showing the level of autonomic activity when both simultaneous blood pressure and G. S. R. data are available. Using such composite scores, Darling claims evidence for two reaction types, one sympathico-adrenal, the other parasympathetic-cholinergic. Individuals whose reactions were preponderantly cholinergic, or possibly strongly balanced between cholinergic and adrenergic, were "more alert, more excitable or non-inhibited, and more active physically or less sluggish" than those whose reactions were preponderantly adrenergic. Factor analysis of the data was in agreement with the above conclusions (12). The many studies of Darrow, Gellhorn, and their associates are indicative of the present promise of straightening out the tangled relationships inherent in the visceral response picture (18, 33, 34, 86).

In studying breathing responses in emotional situations, Gaskill and Cox (31) employed the method of analysis of variance and covariance. Their results indicate the presence of patterns, but no plain indicator of emotion. The new statistical techniques of Fisher (25) seem exceedingly appropriate for the investigation of emotional response, and it is to be hoped that more investigators will follow the lead of Gaskill and Cox in their use.

Whitehorn, Kaufman, and Thomas, while recognizing the complex causes of cardiac acceleration, have made use of it as an emotional indicator with the word-association technique (90). Their figures for the latency of the response indicate that the immediate acceleration is due to an inhibition of vagal action rather than a positive sympathetic innervation.

This has been confirmed by Beebe-Center and Stevens, working with cats and dogs (4, 5). The striking correlation between the magnitude of the heart rate and the violence of accompanying overt movement leads them to suggest that cardiac acceleration is present only when there is either bodily exertion or a "set" in this direction. They regard the increase in pulse rate as part of a general exertion syndrome rather than one element in a unique

emotional response, and cardiac acceleration thus becomes an indicator of effort or exertion rather than emotion. Emotional responses might thus become mere "special cases of violent action" (5, p. 256). The participation of autonomic mechanisms during bodily exertion is a known fact and one example of the nonspecificity of this system. An adequate neurological basis for distinguishing, or identifying, separate syndromes for "exertion" and "emotion" is lacking at present, and at our current level of knowledge Beebe-Center's argument appears rather largely terminological. I cannot see important consequences arising at present from the substitution of "exertion syndrome" for "emotion." One might even argue that all cases of bodily effort are simply special cases of emotion. It is of interest to note, however, that during the first World War one of the many psychoneurotic manifestations appearing among the military casualties was an abnormal physiological reaction to effort. This Effort Syndrome, consisting of a heightened autonomic sensitivity during exertion and containing cardiac acceleration as a prominent feature, was classed as "the physiological result of an emotional state" (69, p. 52). Berg and Beebe-Center (7) have continued the studies of Beebe-Center and Stevens, using human subjects.

The galvanic skin response remains in use as an emotional indicator. It is not specific to emotional stimuli alone, nor is it an infallible correlate of them, but it does seem to appear more often and with greater magnitude following emotional stimuli (63). Despite its imperfection, its use in many studies would indicate some practical value for it as an indicator (*i.e.* 20, 81, 85). Darrow has made an intensive study of the various bodily factors influencing the G. S. R. (17).

While the drop in skin resistance is still considered to be a function of the activity of the sweat glands, the accumulation of evidence now indicates a parasympathetic, as well as sympathetic, control for these glands. Darrow has marshalled the evidence for this view (15, 16). It would thus seem that the conclusion of Landis and Hunt (63) that the G. S. R. may be interpreted as indicating sympathetic activity had best be amended to include parasympathetic action. Such an interpretation would help to explain the confusing nonspecificity of the response.

Working with the pure electrical potential obtainable from the skin without any external current source, instead of the change in skin resistance to the passage of an external current (endo- rather

than exosomatic current), Forbes and Bolles got evidence of two distinct potentials which they call *alpha* and *beta* (28). The *alpha* potential occurred as a correlate of innocuous stimulation and seems to be an indicator of "normal, unexcited reactivity." The *beta* potential, however, occurred most often with stimuli of an "exciting" nature, such as shock, apprehension-inducing situations, and affective and emotional associations. Forbes and Bolles indicate that the *beta* response may be connected with sweat gland activity, which is controlled by the sympathetic nervous system. Since there is indication of nonsympathetic control of both sweat glands and the vasodilator fibers in the posterior roots, Forbes (27) feels that the *alpha* response may reflect one of these.

Further evidence for parasympathetic action in emotional responses has come recently from Gellhorn, Cortell, and Feldman (32). They have produced sham rage in cats by electrical stimulation of the left mamillary body of the hypothalamus and found it accompanied after elimination of the sympathico-adrenal system by a definite hypoglycemia, provided the vagi are intact. In cats in whom the sympathico-adrenal system has been eliminated, a rage response elicited by a barking dog produces a fall in blood sugar. Sectioning of the vagi below the diaphragm abolishes this reaction. The authors conclude that "the normal emotional process as well as the sham rage reaction is characterized by a simultaneous discharge over the vago-insulin and sympathico-adrenal system." In the normal animal the latter predominates and conceals the action of the former.

The complications in autonomic response are no more than careful consideration would lead one to expect. The nonspecificity of autonomic response is not surprising in a nervous system so widespread and so intimately connected to the central nervous system. Indeed, it would be surprising if the autonomic reacted solely in "emergency" situations and was not innervated in the course of ordinary behavior. The interrelationships and automatic-balance functions within the system also add their bit to the confusion of the final response picture in any effector. Moreover, the opposed innervation of the sympathetic and parasympathetic branches where they both supply the same organ does not necessarily imply a conceptual antagonism so complete as to preclude simultaneous action.

Indirect evidence of concomitant sympathetic and parasympathetic activity may be found in studies upon the human infant

where there are reports of simultaneous sacral and sympathetic activity. Clarke, Hunt, and Hunt report that a sudden revolver shot often caused penial tumescence in young infants (11). Halverson has noted tumescence in infants during the general excitement attendant upon thwarting or difficulty in feeding (46). The relative lack of development of the infant's nervous system perhaps renders these cases of dubious value in discussing adult behavior. It is my opinion, however, that the antagonism between sexual responses and general sympathetic excitement is overrated, and that the two may occur together frequently. This would be in accordance with our increasing knowledge of instances in which both sympathetic and parasympathetic activity are present. Such simultaneous action would seem to exist in many of the perverse sexual behaviors reported in the clinical literature. Where inhibition of sex activity due to unpleasant emotion has been reported, the inhibition would not seem to be due to any mutually exclusive antagonism between the two branches of the autonomic nervous system, but rather to the "distracting" nature of the stimuli arousing the supposedly inhibiting emotion.

The relation of the autonomic nervous system to the clinical concept of "emotional stability" is not clear, largely due to the vagueness of this concept. There is no doubt that autonomic response enters into many of the accepted instances of unstable behavior and that the sensitivity and lability of this system underlie one aspect of such behavior. Emotional stability, however, involves more than the mere responsiveness of the autonomic system. As the words are used clinically, they imply an evaluative approach that goes beyond the study of mere behavior into a consideration of such aspects as its biological value and its social fitness. Emotional maturity, which is closely linked in the literature with stability, goes even further and discusses the alteration of autonomic responses through learning, their partial control through the understanding and manipulation of stimulus conditions, and even a disciplinary approach involving the suppression or concealing of their effects.

Moreover, that emotional stability includes much more than mere autonomic response an examination of any of the current paper-and-pencil tests for "emotionality" will reveal. Thus the Bell Adjustment Inventory includes as indicative of emotional instability such diverse things as getting excited, blushing easily, feeling that someone is hypnotizing you and making you act

against your will, and getting discouraged easily (6). It is obvious that there is no single, unifying principle here.

Farnsworth has run correlations between four tests which have been used to measure emotional maturity in adults and finds no significant interrelationships (24). He concludes that "the tests are not measuring one common variable." The increasing tendency to apply factor analysis to personality tests (*i.e.* 42) offers us the hope of more light in the future, but until then it is difficult to relate these concepts to the specific experimental data surveyed in this paper.

In the field of comparative psychology, the concept of emotional stability has been related more definitely to the autonomic nervous system. Hall has continued his studies of emotionality in the rat, using frequency of defecation as an indicator (43). He finds that emotionality can be inherited and suggests autonomic and endocrine mechanisms as its constitutional basis (44, 45). Anderson has intercorrelated various measures of emotion in the rat, and the positive, though low, correlations give support to the claim for the existence of a trait of emotionality (2). He also finds some proof of an endocrine factor being involved (3). O'Kelly has questioned the validity of defecation as a measure, as well as the existence of emotionality as a trait *per se*, raising the same questions of trait consistency that have proved so troublesome in the study of human personality (75).

Billingslea has shown that the data yielded by such studies depend intimately upon the techniques of experimentation, with changes in experimental setting, procedure, and selection of data causing wide variation in results (8). This suggests the same complexity that is found in the study of human emotions and indicates that the sophistication that has been won at great pains from the study of emotion in man might well be applied to the animal field. In dealing with so complex a problem, our human failure to comprehend the complete psychological field of the rat is a handicap that demands caution in our interpretations. In view of our lack of insight into the complete situation, the fixation upon a few simple measures like defecation often lends a spurious air of simplicity and validity where neither may be justified.

In summary, then, the work of the last few years has not increased the specificity of visceral responses as indicators of emotion, but has increased our understanding of the complexity of organization responsible for this lack of specificity. Complete understanding and perfect prediction as a scientific goal remain far away. Indeed, considering the intricate mechanisms involved and the numerous stimulus conditions affecting them, one might

question the possibility of ever achieving a more exact control over, or a better prediction of, human behavior in emotion. The recent work on the autonomic nervous system is revealing so many factors, so many necessary stimulus conditions to be taken account of, that it may prove possible to write the equation for emotional behavior only in the abstract, in the textbook, conceptualized "average" or "normal" man of Wundt and the originators of experimental psychology, and never in terms of an actual human being reacting to a multiform stimulus field at some point in his unique history.

PERIPHERAL RESPONSE

By peripheral is meant the overt, organized, more-or-less patterned behavior primarily effected through striped muscle and the central nervous system. This is the field of the "instinctive" behaviors of an earlier day, a field that today comprises mostly the study of facial expression and expressive gestures.

Here again there has been an increasing awareness of the complexity of the behavior involved. This time the confusion is a matter of the alteration of possible innate responses by the later learning of the organism, one aspect of the controversy continually arising over the "innate" *vs.* the "acquired." The most important trend here is the clear recognition that whatever may be their innate bases, if any, the bodily gestures and facial expressions of emotion are largely culturally acquired. Their purpose is communicative and social, and their study is being taken over by social psychology, not as secondary, explanatory material in the way that social psychology has previously used "emotion," but as primary data of immediate social interest. This is seen in the fact that the best recent summaries of facial expression are to be found in textbooks on social psychology. A particularly good treatment is contained in Klineberg's new *Social psychology* (56).

The problem is posed very clearly in the field of facial expression in emotion. On the one hand we have the voluminous literature demonstrating that consciously posed emotional expression can be interpreted with some success by untrained observers. The ability seems to improve with age, intelligence, and training (62). On the other hand we have the studies of Landis showing chance results for interpretation of photographs taken during emotional situations and indicating that the objective analysis of such photographs fails to show uniform facial patterns of the classical sort

(59, 60, 61), as well as the studies of Sherman showing that observers were unable to interpret either the facial expressions or vocalization of infants unless there was knowledge of the stimulus conditions (82, 83). The present interpretation of these results is that the classical facial expressions of emotion are not innate, but are cultural accretions learned by the organism in the course of his social experience. In mild emotional states, these classical patterns may still appear, but in the stronger states they are overthrown and can no longer be found. The origin of these communicative gestures is still in doubt.

Arguing that innate, reflex responses seem to have a shorter latent period than learned responses, Landis and Hunt proposed a temporal analysis by ultrarapid photography, a technique they call the "magnification of time" (64). Guided by an earlier study of Strauss (87), they studied the facial pattern following a startle stimulus and found a uniform facial pattern appearing in all their normal subjects. The eyes close, and there is a widening of the mouth as in a false grin, although the teeth are seldom bared. At the same time the head and neck are brought forward. The startle pattern is very rapid, beginning about .05 second after the stimulus and often being completed by .20 to .30 second. This primary response is followed immediately by secondary patterns of the usual social sort.

This not only demonstrates the existence of both innate and learned facial expressions in the startle situation, but shows the possibility of separating them by an analytic temporal technique. It is interesting to note that the same startle pattern is also found in apes, although the work of Foley (26) would indicate that the typical, socialized expressions of man are not found.

In an experiment which would be one of the most important in the field if one could grant the author's premises and conclusions, Munn (72) claims results which negate the findings of Landis and of Sherman. Munn objects to previous work on two grounds: that posed expressions of emotion contain a strong element of convention, and that where unposed photographs have been used they have been taken in laboratory situations which are mild as compared with those of everyday life.

With the first criticism every psychologist would agree. The second one is the conventional criticism always raised against "laboratory" emotions. In general it is probably true, but I doubt that it can be raised against the stimulus situations used by

Landis. A careful examination of Landis's stimuli would reveal that some of them, such as the decapitation of a live rat, are undoubtedly stronger than the emotional situations of our civilized, nonlaboratory lives. Nor can the true strength of the stimuli be adequately conveyed verbally in many cases, *i.e.* in contrast to the mild "art" studies so often used, the lewdness of Landis's sexual photographs renders it impossible either to describe them in print or to reproduce them. However, if we grant Munn's criticism, it remains for him to use photographs of facial expressions which are lacking in the conventional elements typical of the posed, socialized expressions and which were taken in emotional situations of great strength.

In order to fulfill these conditions he used candid-camera shots taken from *Life* and *Look*. Some 14 of these, including a girl photographed over a transom while bathing, a man holding the hand of a drowned person, a man with his hand stretched toward a hostile crowd, and a girl running from a "ghost," were judged by some 155 subjects. The results show a striking uniformity of interpretation by the subjects in many cases, the results being better than previous studies would have led one to expect. Moreover, the judgments often were not improved when they were made on the basis of the total photograph as opposed to the face alone. Munn concludes that "the results suggest that spontaneously aroused emotional expressions may be interpreted with as much agreement as are the more conventional posed expressions used in previous research" (72, p. 338).

The representational adequacy of the candid-camera technique might be challenged (54), but there is little indication that Munn's photographs answer his criteria of spontaneity without conventionalization and of strength of the emotion. Most of them involve social situations of a communicative type where conventionalized expression would be expected, *i.e.* the girl "caught" in a bath tub, man gesturing toward the crowd, girl being frightened on a "ghost" party where she expected to be scared and where "being frightened" was an integral part of the proper social conduct. Indeed, even the man holding the drowned person's hand seems aware of the camera. Moreover, one may doubt whether the young lady at the ghost party was badly frightened or whether any young lady in a position to be surprised in a bath would be very upset by its happening.

There is an additional experimental error in Munn's study, the

factor of selection. In selecting his pictures he used certain criteria such as anonymity of the subject photographed, face visible, no racial characteristics, etc., but the factor of agreement with social stereotype apparently is not controlled. There is no guarantee of the necessary truly random selection. One suspects that Munn, possessed of the known human ability to interpret conventionalized facial expressions, selected "good" ones which his subjects would then be bound to recognize.

Under the circumstances, Munn's conclusion that there are uniform spontaneous emotional expressions not attributable to social convention does not seem valid. He *has* demonstrated that with selected materials, conventional expressions can be recognized with more success than had been indicated previously. He contributes nothing to the question of the existence of innate facial patterns and their relation to the demonstrable ones of social communication.

Landis and Hunt have studied the bodily response in startle and found a general flexion pattern accompanying the facial expression (64). This startle pattern begins at the head and moves downward to include raising and drawing forward of the shoulders, abduction of the upper arms, bending of the elbows, pronation of the lower arms, flexion of the fingers, forward movement of the trunk, contraction of the abdomen, and bending of the knees. The response is present from early infancy on in both negroes and whites and is found in primates and other mammals. With repetition of the stimulus wide individual differences in habituation appear, but habituation is never complete. It is an involuntary response and is not amenable to voluntary facilitation or inhibition except under hypnosis. The only significant changes so far recorded occur in catatonia and in some cases of epilepsy. In connection with overflow theories of emotion, it is interesting to note that it still appears even when the startle stimulus serves also as a signal for gross bodily movement. The fact that it is not merely an auditory reflex, but may be released by sudden intense stimuli in other sensory modalities, has already been noted. As in the case of the facial pattern, it is followed immediately by secondary movement of a learned, socialized nature. Goldstein views it as a form of flight or protective reaction and considers it a normal response indicative of the maturity of the higher nervous centers (36).

Other peripheral expressive patterns have received some attention. Fairbanks and Pronovost find uniformities of pitch level and rapidity of

pitch change in the vocal simulation of emotion (23). Kline and Johanssen (55) and Carmichael, *et al.* (10) have studied the role of gesture. Klineberg gives an excellent analysis of racial differences in emotional expression (56, pp. 166-202). All these studies fit into the interpretation given above to the facts of facial expression and offer no serious theoretical implications not inherent in that problem.

The answer to many of these questions once seemed to lie in developmental or genetic studies, but the increasing comprehension of the possibilities of maturation has robbed such studies of much of their decisiveness. An intelligent, fairly recent summary of the work in the developmental field may be found in Munn's *Psychological development* (71).

We have come a long way from the easy optimism of Watson's pioneering studies of the emotional behavior of infants. Yet it is surprising how excellent his observations were and how true they remain, provided one accepts the naïve level at which they were made. Much has been said of the subsequent refutation of Watson's claim that loud sounds produce fear responses in infants, and it is true that if one defines "fear" as some sort of withdrawal behavior (including crying) many exceptions will be found. If one consults Watson's original protocols in the light of our present-day information, however, it becomes clear that he was including under "fear" (a) the Moro reflex, (b) the startle pattern, and (c) a mass of undifferentiated responses including withdrawal, crying, etc., an inclusive definition which does include all the infant's responses to loud sounds, some of which are present (with rare exceptions) in all reactions to such stimulation (89).

Moro reflex and startle pattern have been described adequately. The "mass of undifferentiated responses" still awaits complete investigation and classification, but there is every indication that its study will reveal further patterns of response (53).

The study of such *patterns* encounters certain difficulties due to current trends in the study of infant behavior. One of these is the tendency, as in some of Gesell's work, to concentrate upon the development of individual reflexes. Another is the tendency, as in many of the Ohio State studies, to use analytical techniques of observation which practically preclude the discovery of patterns of integrated responses. Moreover, there is extreme confusion in defining the level (*i.e.* molecular level of behavior, molar level, or purely conceptual level) at which such terms as "withdrawal" are to be studied. By withdrawal one might mean the reflex withdrawal of a limb from a pin prick, any locomotor behavior remov-

ing the organism from the vicinity, or any behavior indicating displeasure or dissatisfaction with the stimulus. A similar situation has developed in affectivity concerning the identification of "acceptance" and "rejection" behavior (52, pp. 824-825). Finally, there is an increasing tendency to work with higher-level concepts, such as frustration, intelligence, intelligence quotient, etc., to the neglect of the investigation of concrete behavior patterns.

CONSCIOUS CONTENT

The conscious aspects of emotion, the "feelings" or experiential "qualia" which accompany the emotional response, have long fascinated psychologists. This fascination has been sufficient to motivate great amounts of discussion and theoretical treatment on a verbal level, yet seldom sufficient to motivate any genuine experimental treatment within the laboratory. Psychology seems willing to accept and discuss the subjective aspects of emotion, but unwilling to attempt a scientific treatment of them. The reason for this paradoxical state of affairs goes deeper than the mere tendency of theory to transcend fact. The influence of behaviorism has left us with a profound distrust of consciousness and its introspective investigation. We prefer the "objective" to the "subjective." Yet each and every one of us seems doomed to the common-sense recognition that we are conscious at times. Our peculiar solution of this difficulty seems to be the complete, naïve acceptance of experience, but the refusal to treat it as a scientific datum for observation and description.

The amusing result is that psychologist, physiologist, and neurologist alike show preference for the objective approach through a study of behavior, neurohumor, or thalamic lesion; yet no sooner do they find some unique aspect of their material than they proudly offer it as the possible basis for the experience of emotion, an experience whose existence, uniqueness, and characteristics are still largely a matter of supposition.

As soon as the endocrines became firmly established in psychology we find Gray proposing an extension of the James-Lange theory, which locates the sources of the "emotional feelings" in the proprioceptive stimulation caused by the endocrine changes in the chemistry of the blood (38). The electroencephalograph has just given us a new datum, the "brain wave"; and since certain peculiarities in these brain rhythms are observed during emotion, Hoagland and his collaborators suggest them as "the cortical signal of

some conscious correlate of the emotional response" (48, p. 261).

If psychology is going to maintain this interest in consciousness, it had better admit consciousness to some kind of scientific treatment. Strange as it may seem, the existence of some unique, specific experiential content in emotion has never been demonstrated adequately. All parties in the famous James-Lange controversy have assumed the existence of a unique and universal emotional quale, and then proceeded to quarrel bitterly over the origin of what may, after all, prove to be merely an hypothesized entity.

James and his opponents all assumed a univocal meaning for the word "fear"; they assumed that where two people labeled their experience by the same term, it must be the same experience. The work of Cantril and Hunt on adrenalin shows the falsity of this point (9). Adrenalin may or may not produce an emotion in the sense of an observer's report of emotion, but as far as I have been able to ascertain during a great deal of work with adrenalin, it does produce the same (roughly) kinaesthetic pattern of sensation in all subjects. They show great agreement in this. The difference seems to be that some people are willing to call this emotion, while others are not. Thus viewed, the entire James-Lange controversy becomes a verbal misunderstanding, a squabble over definition.

To attempt to resolve the entire James-Lange controversy in this way may be unduly optimistic; but there is other evidence for the ambiguity of emotional terms of report that suggests that the mere appearance of common terms of report in the protocols of two different observers is insufficient reason for the assumption that there is a common experiential content behind these words (49). If this is not true, if there is a universal, unique, isolable, and identifiable experiential content peculiar to emotion, then let us find it and describe it, let us study it scientifically. Or if our antipathy to subjective data is too great to allow this, let us extend our antipathy to include a ban on its theoretical discussion.

The possibilities for such a *quale* in experience are many. There is a wealth of kinaesthetic and other sensory material present, and a wealth of ideational material as well (9). This can be investigated, as Nafe and his collaborators have attempted in the field of affectivity (73). That the phenomenological approach will yield reliable data when applied to emotion has been shown by Hunt (51), who asked observers in a startle situation to select descriptive

terms applicable to their feelings from a list presented to them. The terms selected showed great consistency between large groups of different subjects. The same consistency appeared when the observers were requested to apply these descriptive words to other emotions which they remembered from past experience. Professor Schlosberg reports the same results in three years of duplication of this experiment in his experimental psychology classes (79).

In another discussion of the subjective *quale* of emotion, Hunt (50) states that the only universal element in any emotional situation is the use by all the subjects of a common term of report, *i.e.* "fear." That is, while stimulus conditions and actual experiential content may vary from subject to subject, all decide upon the emotion and give it a common label, "fear." Thus the *quale* would be correlated with the appearance of a common implicit verbal response. There are other possibilities as well. Since the subject judges the situation, decides upon its significance before he recognizes the emotion, the *quale* might be some *Bewusstseinslagen* like the feeling of "familiarity," "recognition," etc. accompanying judgment. These are complicated matters and possibly not worth our scientific time, but unless we are ready to deal with them we had best drop the assumption of a unique consciousness from our discussions of emotion.

One concluding caution concerning the James-Lange controversy should be entered here. The previously mentioned evidence of parasympathetic activity in many emotions which were thought to be exclusively sympathetic in function casts doubt upon the decisive nature of the adrenalin experiment in settling the argument. In the past the objection has often been raised that the establishment of a purely visceral response pattern was no fair test of the theory, since James himself included peripheral responses as an important element in the total reaction responsible for emotion. Today we might go even further and object that the visceral syndrome instated by adrenalin is not adequate in itself, since adrenalin is a sympathicomimetic drug and does not furnish the parasympathetic elements that modern research indicates are a necessary part of the complete picture. *Experimentum crucis* or not, however, the adrenalin situation still offers a suggestive approach to the whole problem of the emotional consciousness.

THE HYPOTHALAMUS AS AN EMOTIONAL CENTER

The hypothalamus seems firmly accepted in psychology as the "center" for emotions, but the data still fall far short of adequacy

in establishing this beyond doubt. Lashley (65) has written an excellent criticism of the material, pointing out the need for extreme caution. Hunt (52) has questioned the accuracy and suitability of the introspective material in many of the clinical reports. An excellent survey of hypothalamic function may be found in Volume 20 of the *Research Publications of the Association for Research in Nervous and Mental Disease* (29).

The material on the hypothalamus as a center for emotion comes from a study of both animals and men. With animals the older material on extirpation has been added to by experiments showing the production of emotional responses by direct electrical stimulation of the hypothalamic region (1). We have already mentioned the work of Gellhorn, Cortell, and Feldman in producing "sham rage" in this fashion.

The clinical material on human beings has been likewise extended. Grinker reports the production of anxiety feelings with electrical stimulation of the hypothalamus (39, 40). Hoagland and his collaborators, in dealing with the delta index of the electroencephalogram as an indicator of emotion, report that delta waves from the hypothalamic region accompany those found from the cortex during emotion and that their appearance in the hypothalamus precedes their appearance in the cortex by some four milliseconds (48).

There can be little doubt that the hypothalamus is one, and an important one, of a series of stations connected with emotional expression. That it is *the center*, however, is still in doubt, and the complexity of the problem of emotional behavior casts serious doubt upon any hopes of an explanation in terms of the functioning of a single center. After a careful survey of the pertinent material, Alpers concludes: "It is one of a series of such areas and its relative importance cannot as yet be gauged" (1, p. 741).

EMOTION AND LEARNING

While much has been said about the opposition of emotion and reason and about the interference of emotion with the processes of learning, the material does not offer much that is pertinent to the purposes of this paper. Too often it has been discussed in a somewhat popular, casual fashion, e.g. Metfessel's recent article on the all-or-none nature of emotional thinking (68). A great deal of it has been handled at a clinical level with reference to concepts such as repression, wish-fulfillment, etc., which lie beyond the more narrow experimental interests of our treatment here. Still more

of it has been done in relation to the Law of Effect and the influence of hedonic tone. These studies lie within the field of affectivity, outside of emotion proper.

In the remaining work one significant trend appears. This is the tendency to view the effects of emotion on learning not as due to some peculiar characteristic of emotion *per se* acting immediately on the learning process, but rather as attributable to the "distracting" nature of the emotional situation. Thus Patrick has shown that if one uses Hamilton's criteria for the evaluation of types of response in the multiple choice situation, there is a marked diminution of the relative number of type A, or "rational inference" reactions, under emotional conditions (76, 77). He attributes this to the fact that both the stimulus conditions and the resulting emotional responses act as distractors in the learning situation. Gagné and Graham, in working on conditioning in rats, have reported that under emotional conditions there is a retardation in the appearance of the typical curve for the acquisition of a conditioned response (30). Here again the retardation would seem to be due to the distracting nature of the stimulus conditions in the emotional setting and the possible interference of the uncoordinated responses called forth.

THEORETICAL APPROACHES TO EMOTION

Emotion has always been a fertile field for theoretical treatments, and the last few years have been no exception. There seems to have been less novelty and less progress here, however, than in the experimental approaches. What has been done has been more in the rephrasing and restating of older views rather than in the introduction of new concepts and the suggestion of new relationships. At the beginning of this paper we stated what seemed to be the usual implicit working definition of emotion, the general conception that has held the field together. It contained the idea of emotional situations as emergency situations of biological importance to the organism in which current behavior is suspended and there appear patterned responses directed toward a resolution of the emergency. Current contributions follow the classical ones in exhibiting three more or less distinct trends: (1) They may concern themselves with the specific mechanisms whereby current behavior is interrupted and "emotional" responses are substituted. "Overflow" and "thalamic" theories belong here. (2) They may evaluate the place of emotion in the total biological economy of the

organism. Here we find the teleological or purposive interpretations. (3) They may discuss the implications of emotion for other fields, such as education.

Under the first category we will mention articles by Darrow and Koffka. Darrow deals with emotion as a functional decortication (14). When there is a dynamic cortical conflict with opposed demands for action, the normal cortical control over subcortical mechanisms is partially broken, and behavior is now subcortically controlled. Here again we have the ideas of "conflict" and "overflow" that have been so prevalent in emotion.

Koffka gives a dynamic interpretation of emotion based upon the work of Lewin and his students (66). The psychological field is permeated by forces which either maintain it in equilibrium or, when strong tensions arise, produce change. Such tensions may arise when an object with a positive valence is present in the field, but the organism is prevented from reaching it by the presence of a barrier. When these tensions within the psychological field involve the Ego (as a subsystem of the total field), emotion results. If the tensions become strong enough, an "explosion" results and emotional behavior follows (58).

This treatment serves to bring emotion within the broad systematic framework of Gestalt psychology. Despite the novelty of the particular verbal formulation used, the main idea would seem to be the old one that when an important activity of the organism is blocked, emotion follows. Mere novelty of presentation, however, is not enough; and a final evaluation of the Gestalt point of view will have to wait until we have had time to estimate its stimulation value for the experimental laboratory. When we can state *what* objects have *how much* valence, *what* barriers of *what strength* exist, and *what specific forms* the exploding energy takes, we truly will be advancing toward a fuller knowledge of human behavior.

Koffka seems to recognize this, when, in speaking of Dembo's work on anger, he admits that conflict theories of emotion are no novelty, but claims that "they are all *much less concrete* than Dembo's theory" (58, p. 408; italics mine). Whether Dembo's work is more concrete than Watson's stimulus-response formulation in studying the production of anger in the child through restraint of movement could be questioned. So far, the stimulus-response doctrine seems to have given us the more concrete knowledge of emotional behavior, and Gestalt psychology has not

as yet shown the same richness here that it has in the study of perception.

Under the teleological classification we find Goldstein interpreting emotion in a way that is reminiscent of the psychoanalytic approach (37). For him, anxiety sets in when it becomes impossible for an organism to cope with tasks commensurate to its real nature. The organism fails in its task of realizing its essential nature and faces a dissolution of its world. Fear is the experience of the possibility of the onset of anxiety.

The purposive point of view is also taken by a philosopher, Nahm, who argues for the importance of the teleological interpretation of behavior in an article which takes current psychology to task for its ateleological treatment of emotion. Nahm attempts to remedy this. He states the theory that in emotion there is first a check upon some behavior. After this check the organism becomes aware of the potentialities of the situation, and alternative possibilities of action present themselves. This awareness may be accompanied by a glandular response of readiness. Then the organism acts, either instinctively or not. Emotion is the upset or awareness period, and it functions to allow increased flexibility in otherwise reflex action and to emphasize an increasing freedom from control by nature, substituting control of nature (74).

Nahm's article is stimulating, but the empirical demonstration of increased flexibility of behavior in emotional situations would help it. Until he can produce experimental evidence that emotion does serve to "free" the organism, his theory must remain upon a purely rational level. In the study by Patrick, mentioned above, there was some indication that emotion upset preconceived, stereotyped plans of behavior, but the low quality of the succeeding "substituted" behavior argues against any resulting increase in the "freedom" of action. If anything, in Patrick's study emotion resulted in less ability to cope favorably with the environment. Hall also mentions less stereotyped behavior in his emotional rats, but there is no evidence that it was "better" in any adaptive sense (43).

Only one article will be mentioned under the third category. In stressing the educational implications of emotion, Tuttle refers to it as a substitute response coming only when "no habit is ready, and no plan can be devised" (88). Emotional training is thus not a matter of re-educating emotions, but of preventing them by offering adequate outlet for motivation. Our task is "to educate interests so as to secure desirable impulses, to educate judgments so

as to secure effective solutions; and to *educate skills* so as to assure efficient execution: all for the sake of *forestalling disruptive emotions*."

A final approach which might be mentioned, but which does not fit into the above classification, is that of Schramm (80). He has constructed a "periodic table" of emotions that attempts an orderly classification on the basis of three dimensions, the relative superiority-inferiority of the organism, the favorableness-unfavorableness of the stimulus, and the time and space conditions between organism and stimulus. Thus, repugnance is found when a superior organism is oriented at a point of distant fixation to an unfavorable stimulus, while hope comes when a superior organism has partially approached a favorable stimulus. The classification is a somewhat scholastic performance on a purely rational level, and there is no evidence presented that the psychological states Schramm is dealing with are all emotional or that they have anything in common beyond their inclusion within the "periodic table."

One is left with the impression that in the field of emotion at present the main contribution is experimental rather than theoretical. The greater progress is coming through the constant accrual of knowledge concerning concrete human behaviors in concrete situations rather than through the new formulation of these facts within a wide theoretical framework. This may mean that the experimental investigation of emotion has not as yet caught up with past theoretical formulations, or it may mean that these contributions were sufficiently sound to make any alterations unnecessary at present.

SUMMARY

The concept of emotion has proved to be a vague one. It designates a rough field which has been held together by the idea of "innate responses" in "emergency" situations. Any of the current definitions may be, and have been, challenged. The fact remains, however, that under the stimulus of this concept much experimental work has been done, and many things have been learned about human behavior.

Out of the vast, poorly related mass of investigations undertaken, one field has stood out—the study of the functions of the autonomic nervous system. From its beginnings in Germany as the study of the visceral concomitants of feeling, through its concen-

tration on sympathico-adrenal function under Cannon, to its present enlarged interest in both sympathetic and parasympathetic activity, it has been a lively, active line of research—research in which the psychologist has played a full part. This is not to define emotion as autonomic activity nor to reject this definition. Such wrangling over terms is profitless. It is merely to say that the study of the activity of the autonomic nervous system has been, and still is, a flourishing field for psychological experimentation and a field that has given us much, not merely of interest for the theoretical interpretation of the neurological and neurohumoral bases of human behavior, but of value for clinical guidance and practice.

In the past few years another field seems to have been developing, although it is still in its infancy and its growth is as yet uncertain. This is the study of emotional expression as a means of socialized communication. It may well take its place in social psychology as an important phase of the study of language development.

Nor has the work in emotion failed to contribute to other fields. It has taught us much about the genetic development of human behavior, about the central nervous system, about the existential data of consciousness, and about the so-called higher mental processes.

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RESEARCH ON SPEECH SOUNDS FOR THE FIRST SIX MONTHS OF LIFE

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Nowhere in the literature of the beginnings of speech during infancy can there be found a systematic and comprehensive account of speech development during the first six months of life. Such accounts as exist are sketchy and for the most part non-consecutive. The best statements are by Tracy (21), Preyer (15), the Sterns (19), Lewis (13), and Leopold (12). Observations are restricted for the most part to one or two children of the writers themselves. An exhaustive treatment of the literature of the whole range of infancy will not be attempted here. However, a running summary of reports covering speech sounds during the first six months will be presented.

One of the earliest reports on the order of appearance of vowel sounds is that by Löbische (14). As cited by Tracy (21), this order is: *a, e, o, u, i*. Löbische held that the labials *m, b, p* are the first consonants, an opinion which has cropped up again and again in the literature during the last 90 years. The labials are followed in order by *d, s, g, w, f, ch, k, r, sch*. Preyer criticizes Löbische's sequence as "going far beyond pure observation." Sigismund (18), according to Tracy, found the vowel sequence to be *a, ä, u, ei, o, i, ö, ü, äu, au*. Schultze (16) held that the vowels ought to appear in the order: *Ä, A, U, O, E, I, Ö, Ü*. Preyer (15) admits that *ä* is one of the first vowels, but criticizes Schultze in considering it invariably the first. Tracy (21) indicated the fact that vowels usually precede consonants and believed that, of the vowels, *a* (as in *calm*) with its various shadings is generally the first to appear. He also pointed out that before the sixth month "the primitive vowels are combined with one another and with consonants to produce the first syllabic utterances" (p. 127). In a number of cases the labials *b, p, m* make their appearance. Sometimes, however, *g* or *k* precedes them. *R* and *l*, which usually are the last to appear, are used by some children quite early. The consonant *m*, which, according to Tracy, is an easy one, combined with the easy vowel *a*, yields the combination *ma*, which eventuates into *mama*. "In the case of

the boy A . . . the earliest combination was *mam-mam*, used in crying." He adds: "In some cases nearly all syllables have been correctly pronounced during the first half year; while in others progress is much slower, very few syllables being certainly mastered before the ninth month" (p. 128).

Tracy's treatment of the subject was rather in the nature of a summary of observations available up to the year 1896. In 1898 Preyer (15) published his two volumes, *The mind of the child*. The second volume contains an extended account of language development of his own children during the first three years of life and also a valuable appendix summarizing previous work. The following is an outline of the status of speech sounds during the first six months according to Preyer.

Month	Sounds
1	u—ä (crying)
2	am—ma, ao, ta—hu, gö örö, ara
3	m (frequent), nei, a—omb, la, grei, aho, ma, mömm, ngö, ra—a—ao, nā, nai—n, habu, a—i, uāo, ä—o—ä, ä—o—o, o—ä—ö
4	ntö, ha, löna, nannana, nā—nā, nanna, ä—ü, ävā, ü—ä, ü—ä, ü—ü—ä—ö, amme—a, üä
5	äggëggëkö, gö kö, the consonant k, ägö, ma—ö—ë, hä, ä, ho—ich, aja
6	örrgö, ä—ä—i—ö—ä, eu, oeu, ä, ö, ijä

The above outline, however, does not give an adequate picture of Preyer's very extensive work in the field of language development. It does, nevertheless, present a typical variety of vowel and consonant sounds together with their combinations during the first six months. From observations on his own child Preyer found the order of succession to be: *u, ä, a, ö, o, ai, ao, i, e, ü, oeu, au, oi*. He adds, however, that other children give a varying order.

Die Kindersprache, by Clara and William Stern (19), has for 30 years been considered the outstanding treatise on infant speech. Surprisingly, however, very little original material bearing on development during the first year was collected by them. The few observations during the first six months on their child, Hilda, are:

First few days	ähä
0 years, 1 month, 14 days	krä Krä
0 years, 2 months, 0 days	erre erre
0 years, 2 months, 14 days	ekche, ekche

On Gunther the following were found:

First few days	ä ähä
0 years, 2 months	ä hä, erre
0 years, 5 months	a dadada

Blanton (1), in 1917, published some observations of the birth cry and of speech sounds during the first month.

The birth cries of different infants were not alike, ranging from simple *a* (as in *at*) to *u* (as in *cut*). Most of them were compound *u* (as in *cut*) followed by *nah* (as in *at*); *uh* (*cut*); *nga* (*at*) (p. 458).

Sounds heard during the first 30 days.

Consonant sounds commonly heard are *m* in conjunction with *a* as *ma* (*at*), *n* as *nga* (*nat*), *g* as in *gah*, *b* as in *ba* (*at*), *w* in *wāh* (*at*), *r* as in *rah* (*at*), *r* as in *burr*, very slight sound, and *y* as in *yah* (*at*).

Vowel sounds are *o* as in *owl*, *e* as in *feel*, *oo* as in *pool*, *a* as in *an*, and *a* as in *father* (relatively rare).

Of interest was the variety of animal cries simulated in the nursery. The "pot-rack" of the quail, the cry of the goat, the whine of the pig, and the wail of the wild cat, each had a close imitation (p. 460).

Her curious barnyard theory of infant cries has been echoed by Gesell (7) in his last volume.

Jespersen (11) states:

First, then, come single vowels or vowels with a single consonant preceding them, as *la*, *ra*, *lō*, etc., though a baby's sounds cannot be identified with any of ours or written down with our letters (p. 104).

Babbling or crowing begins not earlier than the third week; it may be, not till the seventh or eighth week. The first sound exercises are to be regarded as muscular exercises pure and simple, as is clear from the fact that deaf mutes amuse themselves with them although they cannot themselves hear them (p. 104).

Concerning consonants he repeats the statement first made by Löbische (14):

All are agreed . . . that among the consonants the labials *p*, *b*, *m*, are early sounds, if not the earliest (p. 105).

Hoyer and Hoyer (9), in 1924, reported a study on the speech development of their child. The first cries after birth were made on the sound *ā*. The following tabulation summarizes the findings of the Hoyers:

Immediately after birth	<i>ā</i>
0 years, 0 months, 11 days	<i>uā</i> , <i>uā</i>
0 years, 0 months, 21 days	<i>ā</i> , <i>uā</i> , <i>māā</i>
0 years, 1 month, 20 days	<i>r</i> , <i>g</i>
0 years, 3 months, 0 days	<i>ma</i> , <i>aggg</i> , <i>ma</i>
0 years, 4 months, 28 days	<i>g</i>
0 years, 5 months, 8 days	<i>b</i> , <i>p</i> , <i>g</i> , <i>m</i>

Gesell (6), in 1925, reported a study by Malmberg on a complete 24-hour record of the vocal activities of a six-month-old

child. Sixty-four different sounds were distinguished. The most frequently recurring sound was *da* with a frequency of 63. *A* occurred 46 times; *ba*, 30; *ngw*, 21; *ana*, 15; *ada*, 13; *uh*, 11; and the remaining sounds occurred less than 10 times.

Decroly (4) believes that babbling begins toward the end of the second month and overlaps the early period of cries. The first babbling sounds are *e* and *m* and especially the gutturals *g*, *k*, *r*.

Bühler (2) discusses at length the emotional quality of early cries, but does not explicitly state their phonemic nature, except to say that modifications of the crying sounds can be observed from the second month on.

Whimpering, sighing, and sounds of groaning make their first appearance at the end of the second month. Sounds of displeasure . . . can be found . . . at the end of the fifth month . . . they are phonetically different from other sounds.

Thus, for example, G 30:0:6 began to use as a constant sound of displeasure *buh* and *erre erre* (p. 38).

Hazlitt (8) claims that the vowels "ah" and "eh" are usually the first sounds and are distinguished even in the crying of the newborn, and repeats the hackneyed expression that the labials *b*, *p*, *m* are early in appearing. The liquid consonants *r*, *l* are usually the last to be sounded. She points out that "by about six months children are employing in their babble approximately all the vowels and consonants that enter into the different forms of conventional language" (p. 51).

Shirley (17), in 1933, published the second of her three volumes. In it she suggests that the earliest vocal sound to be recorded among her subjects was a grunt consisting of a rudimentary vowel such as short *u* or broad *a*, finished by an *ng*. This grunt appeared about the sixth day. She goes on to say that during the first three months syllabic vocalization developed. The earliest syllables were cooings such as *boo*, *goo*, *hauh*, *aah*, *woo*, *xgsoo*, *aak*, *see*, and *voh*. These appeared at 10 months. Somewhat later the following sounds were heard: *ungoo*, *heuhe*, *umwah*, *hu-hu-hui*, *agoo*, *elow*, and *umaah*. After the fifth month repetitive speech consisted of: *uggle-uggle*, *erdah-erdah*, *oddle-doddle*, *a-bah-bah*, *hey-hey*, *bup-bup-bup*, *aduh-duhdeh-duhde-ooh*, *aduh-ajuh*, *awooh-awah*, and *lul-lul-lah*.

In 1938 Lewis's (13) book on *Infant speech* appeared, and in 1939 Leopold (12) published a valuable account of bilingual development of his daughter.

Since the Sterns (19) wrote their volumes on *Die Kinder-*

sprache in 1907, the most ambitious venture in the field has been Lewis's *Infant speech*. It is a critical and comprehensive summary of the whole literature of the subject. Excellent accounts of the individual studies of the speech of young children are presented in the appendices. His own original contribution consists of data on the linguistic development of his own son. The following represents an adaptation of the outline of this boy's speech sounds during the first six months, presented in Appendix A of the book.

Age	Sounds
0 months, 14 days	ũe ue, e, a, ũa, le
1 month, 10 days	ga
1 month, 11 to 13 days	le, le, ne, ne, na, ŋe, ŋe, ŋa
1 month, 14 days	g
1 month, 20 days	g
1 month, 27 days	g
2 months, 2 days	ε, ũε, ŋε, le, je, ʋa, ne, ja, ea, ε, r
2 months, 6 days	æə, ue, εə, xε
2 months, 10 days	əŋaI, aI, ga, εε, gege, ge, gi, geI
2 months, 12 days	ŋgê, ên
2 months, 20 days	εεεε, gege
4 months, 2 days	εεε
5 months, 1 day	bub, bub
5 months, 2 days	ppp
5 months, 4 days	bub, bub, pp
5 months, 6 days	bub
5 months, 7 days	ha
5 months, 13 days	m, m, m

During the interval of the next two months no transcriptions were made of the child's sounds.

A work of somewhat more limited scope than that by Lewis (13) is Leopold's (12) monograph on *Speech development of a bilingual child*, published in 1939. It contains transcriptions on the developing speech sounds of a daughter. The emphasis, however, is upon bilingualism. Some observations on a second daughter furnish supplementary material. Leopold reports that during the first weeks the only sounds his daughter produced were cries of dissatisfaction. "The crying consisted of front vowels between [æ] and [a], usually [a]. During the first week it was clearly [aa:], later as a rule simple long [a:]."

Leopold's findings during the first three months are herewith outlined. No sounds for the second quarter are reported.

Month	Sounds
1	Kx, R, oe, ʌ, d, Il r d, Il g d, l, h I l d F Ru
2	u K x u, E g ə b w, E ə, ʌ, u, v

In a recent volume Gesell (7) has given a description of the vocal behavior during the first year. Excerpts from this account are given.

Except for crying *hA* (the four-week-old infant) is almost inarticulate. . . . His vocalizations are meager and non-expressive, but he mews and makes small throaty noises, precursors of babbling (p. 20).

The 16-week-old infant babbles, coos, chuckles, gurgles and laughs. These are fundamental productions of the oral and respiratory apparatus which will ultimately subserve articulate speech (p. 21).

The barnyard theory reappears in the following quotation:

The 28-week-old infant crows and squeals. At 16 weeks he cooed, at 4 weeks he merely mewed! He has made progress since those small neonatal sounds which emanated from a throat that was used almost exclusively for alimentary purposes.

In the last twelve weeks he has indulged in abundant spontaneous vocalizations, producing vowels, consonants, and even syllables and diphthongs. He is almost ready for defined, duplicated utterance of *mu*, *ma*, and *da*, which lead to his first words (p. 23).

There remains for consideration a series of unpublished studies on the status of speech elements during the first six months of life. Curry and Irwin (3) have studied the speech sounds of newborn infants, and Irwin and Krehbiel (10) have transcribed the sounds of 15 babies during the second quarter of the first year. An analysis of over a thousand vowel sounds heard in the cries of 40 newborn infants shows the following distribution:

Vowels	Frequency	Per Cent
	Front	
i		1
I		6
ε		15
æ		70
	Middle	
Λ		7

The *æ* sound predominates, and *ε* occurs only occasionally. It is interesting to note that an overwhelming majority of these crying sounds are front vowels. Middle vowels are infrequent, and the back vowels *u*, *u* are present in about 1% of the time. This is apparent from the following tabulation:

Vowels	Per Cent
Front	92.0
Middle	7.0
Back	1.0

Curry and Irwin secured an agreement of 94% between their observations.

Consonant sounds during the first 10 days of life constitute only a small percentage of sounds. Contrary to reports in the literature the labials *b*, *p*, and *m* did not occur. The glottal sound *h* is the most frequently used consonant, and occasionally *w* and *k* are heard.

Irwin and Krehbiel (10) have compared the status of front, middle, and back vowels during the second quarter of the first year of life. The data for the sounds made while crying as well as for those made when not crying are included in the following tabulation:

	Month	
	Fourth	Sixth
Front Vowels		
Crying	56.5	70.9
Noncrying	46.5	54.6
Middle Vowels		
Crying	26.1	23.8
Noncrying	29.2	27.0
Back Vowels		
Crying	15.5	5.5
Noncrying	23.9	18.2

An interesting comparison may be made from the data of these two studies. They indicate that, whereas at birth front vowels in the crying of newborns constitute 92% of the sounds, at the fourth month they represent only 57% of this class of vowels, and 71% at six months. Whereas middle vowels constitute 7% at birth, they have increased to 26% at four months; and while the back vowels are practically absent at birth, at four months they amount to 16%.

It is difficult to secure an adequate sample of noncrying sounds at birth, but during the second quarter these sounds are increasingly prominent.

Consonants are infrequently used by newborn infants. The glottal *h*, however, is present. During the second quarter of the first year this sound constitutes about 60% of all consonant sounds uttered. The sound *g* is used about 20% of the time. *M*, *n*, *b*, *d*, *k*, *w*, *l*, *j*, and *e* amount to about 10%, and *t*, *v*, *z*, and *f* are used less than 1%. The remaining consonants rarely occur.

Reliabilities between observers in these two studies are indicated by per cents of agreement well above 90.

It will be apparent from this review of the more important studies of infant speech sounds during the first half year of life that there does not exist a large body of data secured from adequate samplings of infants for purposes of a statistical analysis. Most of the observations were taken on one or two children. Usually no systematic research methods were formulated, statistical techniques essential to the analysis of mass data are practically absent, no reliabilities of observers have been established, many observers used an alphabetical rather than a phonetic system of symbols for recording, and most reports indulge in an inordinate amount of interpretation supported by very little empirical material.

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BOOK REVIEWS

MASLOW, A. H., & MITTELMANN, B. Principles of abnormal psychology: the dynamics of psychic illness. New York & London: Harper, 1941. Pp. x+638.

This book, commendable in many respects, has such grave defects that the reviewer must class it among those books which he considers most unsuitable for students just beginning their study of abnormal psychology. It is commendable in setting up a new line of organization for elementary texts of abnormal psychology. In place of the traditional presentation of disorders of attention, affect, perception, etc., or of the description of the psychiatric diagnostic categories, the material here is oriented in terms of conflict, frustration, self-evaluation, coping with danger, etc.

Great stress is laid on the point of psychodynamics in contrast with the more static viewpoint of former texts of abnormal psychology. By psychodynamics the authors understand the meaning and motivational role of the symptoms as they occur in psychopathology. The subtitle of the book, *The dynamics of psychic illness*, is really more descriptive than is the main title, *Principles of abnormal psychology*.

This presentation is completely oriented around the idea that social and cultural factors determine the quality, quantity, and nature of the illness. Wherever possible, the development of these social and cultural processes is traced back to events in the earlier life (childhood) of the individual. Explanations are usually given in psychoanalytic terms but not the biologically oriented psychoanalysis of Freud. They state their point of departure as follows: "It is the cultural definition of a biological fact that will create the importance or unimportance of this biological fact as a psychological determiner of the personality."

The chapters on psychotherapy are interesting in that they synthesize a rather new and eclectic viewpoint which characterizes the more modern methodology now followed by many psychotherapists, particularly those of psychoanalytic inclinations.

The section devoted to the "Symptom Syndromes" is divided into chapters on traumatic neuroses, anxieties and phobias, obsessions, disturbed states of consciousness and memory, hypochondriasis, organic neuroses, manic-depressive reactions, schizophrenic reactions, organic psychotic reactions, and feeble-mindedness. A bibliography of 819 titles is provided. This bibliography is a working list of references which have actually been used in the make-up of the book. The text is well documented and up-to-date. The suggested readings at the end of the chapters seem to the reviewer to be pertinent to the material and should be useful to the student and the instructor. A glossary of terms and a description of projective methods for psychological examination are well done and are attractive features. The book should be easy to teach, since it is written in an interesting style. The physical production, printing, binding, etc., is unusually attractive.

In spite of all of these new and unusual features the reviewer must urge several vital criticisms, chief of which comes from the fact that this is so completely one-sided in presentation. Environmental, cultural, and social motivating factors are constantly referred to, while biological or physical circumstances are either neglected or pointed out as unimportant. The literature around which the text is built is almost entirely selected as pointing to this one viewpoint. Again, some of the experiments which are cited are uncritically evaluated.

The reviewer feels that this is a deceptively attractive text which would be easy to teach, but it must necessarily lead to the neglect of the possible biological determination of disease (either physical or psychic), so that the student who may do advanced work in psychology or the intelligent layman who continues an interest in this field must of necessity unlearn whole sections of this social theory and reorient his thinking so that it will include biological findings, both old and new.

Our present-day knowledge of psychopathology is so debatable with respect to cause and effect that it is sheer folly to completely adopt either the biological or the social explanation at the expense of the other. Either hypothesis may be correct or incorrect, and at present the student is entitled to know the facts and theories on both sides, since future study and development in this field for many years to come will swing about this debatable point of etiology. Every unbiased worker in this field clearly recognizes that there is no certain or conclusive evidence that psychopathological conditions are uniformly related to hereditary predisposition or to biological anomalies (anatomical or physiological) occurring in the life of the patient. It is equally clearly recognized that there is no conclusive evidence which points to any uniform relationship between cultural or social customs or occurrences and psychic or physical illness. The complete acceptance of either viewpoint at the expense of the other, particularly in an elementary textbook which is laying the foundations of knowledge in the subject, is an act of deception which no author should inflict on the beginning student in this or any other field.

In the opinion of the reviewer, the work and conclusions of both Kallmann and Myerson are misrepresented and misinterpreted. Certainly Myerson attributes the theory of blastophoria to Forel, and Myerson's theorizing in terms of blastophoria was quite different than this text indicates.

Perhaps the choicest sample in this text is the following: "It was a favorite amusement of Dr. Adler to take children who had been diagnosed as stupid or even feeble-minded in terms of I.Q., and by means of psychotherapy, to make them intelligent again." *Verbum sap.*

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BURT, C. The factors of the mind: an introduction to factor analysis in psychology. London: Univ. London Press, 1940. Pp. xiv + 509.

Factor analysts seem to have reached the stage where they want to take stock of the tremendous developments that have grown out of Spearman's pioneer work. Two years ago Godfrey Thomson's *The factorial*

analysis of human ability appeared as the first systematic critique of the whole field. Cyril Burt's *The factors of the mind* and a forthcoming book by Holzinger and Harman are in the same vein.

Burt's book is a combination of three parts originally planned for separate publication: an essay on the logic underlying factor analysis; a comparison of the different methods; and a discussion of the inverted factor technique and its application to the study of temperamental types. An appendix gives Burt's computing methods.

Part I discusses the general logic, as opposed to the mathematics, of factor analysis. "I hope to show," Burt writes, "that, like many other scientific methods that wear a mathematical dress, factor-analysis is merely a refinement of a simple and very ordinary logical procedure" (p. vi). "In my view the *primary* object of factorial methods is . . . exact and systematic description" (p. 13). Factors are "simply principles of classification" (p. 97) which provide this systematic description by *organizing* the data analyzed, and organization, to Burt, is more important than simplification and more important than finding interpretable factors. Consequently, he rejects those systems which have been offered on bases of simplicity or meaningfulness. He prefers unrotated, bipolar factors of the principal-axes type, a method of factoring identified in this country with the names of Kelley and Hotelling. In view of his primary aim, Burt's preference is fully justified; no other method provides as accurate and systematic classification of the total variance of a set of scores. Even so, most factor analysts refuse to adopt it, for most of them are interested in achieving a parsimonious set of factors, in obtaining factors which are psychologically meaningful, or in finding the common factors presumed to underlie the varied performances in a given area. Burt minimizes these objectives.

This section on fundamental logic is the most important part of the book. In discussing the meaning and limitations of the coefficient of correlation, in warning against the logical fallacies which sometimes ruin a factor study, and in showing where factor analysis should not be used, Burt's arguments provide good insurance against ill-advised factor studies. On other points, Burt's conclusions will sometimes be rejected and his reasoning sometimes criticized. Even where this is true, attempts to answer his challenging arguments will force a clearer understanding of the limitations of factor analysis.

Part II discusses the relations among different methods of factor analysis. Except on special points, Burt's treatment is less satisfactory than Thomson's. The methods of Holzinger and Tryon are scarcely mentioned. Thurstone's method is occasionally misrepresented, as when Burt writes that "Thurstone admits no 'general factors' whatever" (p. 297).

A major aspect of Burt's treatment of the different methods is his serious attempt to resolve their differences. The factors isolated by different methods, he concludes, "prove to be either linear transformations of, or approximations to, one and the same set of values" (p. 365). Although many other factor analysts have been more aware of the similarities than Burt admits, the point is one which deserves emphasis, and it is well to have it made again.

The section on the analysis of temperament by means of the inverted factor technique reviews Burt's own pioneer attempts in this field and compares his methods with those recently advocated by his friend and colleague, William Stephenson. Stephenson's papers seem to Burt less revolutionary and original than they do to Stephenson. This section will appeal to a limited group, since the inverted technique has not been widely applied. Anyone considering using it should study Burt's views in parallel with Thomson's criticisms of them.

It is natural to compare the volume by Burt with the one by Thomson. Both represent the views of critically-minded authorities on factor analysis. Both will appeal to the same audience. Both emphasize methods more than results. Both authors recognize Thurstone as the leader of the American developments in the field, and both are skeptical of some of his claims for the value of rotating axes. Thomson, primarily a critic and not the advocate of a particular method, presented a thorough, impartial, and better critique of the different factor methods. Burt, always more the practical factor analyst than the critic, offers a vigorous exposition of his own views and methods and an able essay on the logic of factor analysis.

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REXROAD, C. N. Psychology and personality development. Boston: Christopher, 1940. Pp. 501.

It is no secret to psychologists, and probably not to the general public, that there is enormous difference of opinion as to what an elementary course in psychology should contain. A comparison of the Tables of Contents of the dozen most recent textbooks would shock anybody but a psychologist, so vastly different are the subjects included and the varying emphases placed upon them. It should not be assumed that these differences are necessarily a bad thing. They do not indicate that psychology is dying or even sickly. They do indicate that psychologists believe strongly in self-criticism and are not afraid of new methods.

If the elementary textbooks which have appeared in the last few years have been written from as many viewpoints and with as many different arrangements and emphases of content as there are writers, this merely continues a development which originated when psychology first began to object to being termed mental philosophy. This development has not been in a straight line by any means, but has unfolded along many diverging lines.

The reviewer is dated when he states that the elementary textbook which he read in college was Stout's *Manual of psychology*. (Incidentally, it would be interesting to observe the reaction of an average sophomore class to Stout today!) A comparison of the subjects discussed by Stout with those discussed by Rexroad brilliantly illustrates how far psychology has progressed—or, at any rate, has moved.

Stout used 120 pages to discuss sensation, devoted nearly 100 pages to perception, which included learning and emotion, another 100 pages to the growth of perception of the external world, and nearly 200 pages to the ideational and conceptual processes. A few paragraphs sufficed him

for discussion of the nervous system and sense organ anatomy, and students who wished more detailed information were referred to appropriate textbooks of anatomy and physiology.

Rexroad, whose book is scarcely half as long as Stout's, has 75 pages on learning, 30 on the nervous system, 40 on primary emotions, 30 on the inferiority complex (of which Stout was fortunate enough never to have heard), 50 on heredity, 40 on determiners of action, 48 on perceiving, 25 on thinking, 24 on tests and measurements, and 35 on personality development, improvement, and psychological maturity. One chapter of 10 pages deals with the problem of determinism, on which subject Stout has a chapter of 40-odd pages. And very different chapters they are!

Such utterly dissimilar books are convincing evidence of an almost epochal alteration of viewpoint or, possibly, of a growing confusion of viewpoint. Whatever one thinks of Stout—and the reviewer, for one, would never dream of using it as a textbook today—he certainly knew what he was trying to do. Some recent textbooks, among which Rexroad's should perhaps be listed, give the impression of not being quite sure of the boundaries of the subject.

It is said that Professor Eddington once remarked, when discussing theories of light, that on Mondays, Wednesdays, and Fridays he spoke of light as ether vibration and on Tuesdays, Thursdays, and Saturdays he considered it as particle emanation. The reviewer confesses, somewhat shamefacedly, that sometimes he defines psychology to his classes as the Science of Personality or something of the sort and sometimes as the study of the Art of Living.

Any teacher who happens to be in the mood to consider psychology as the study of the Art of Living might find Rexroad's a very useful text. The book, evidently written for rather immature students, is full of good advice on how to study effectively, how to control fear and anger, get rid of an inferiority complex, and escape from the perplexities of philosophical determinism. This remark is not made in a derogatory sense. Such advice is worth giving and, for those who can take it, well worth receiving. Yet, surely, even an elementary textbook in psychology should to some extent present a system.

The reviewer, while of the opinion that the members of most sophomore classes are not quite as immature as the tone of this book seems to suggest, yet finds the book as a whole very interesting. Its arrangement of topics is refreshing in its novelty, and it may be that some of the subjects left out or briefly dismissed can be spared perfectly well.

How useful the book will prove in actual use is hard to say. Any prophecy on this point would merely reflect the convictions, or the prejudices, of the reviewer as to what constitutes the field of psychology. Perhaps the only way to find out is to wait until the book has been used in various institutions for a few years and then arrive at a collective verdict.

GEORGE ROSS WELLS.

Hartford Seminary Foundation.

FRIES, C. C., with the coöperation of A. A. Traver. English word lists: a study of their adaptability for instruction. Washington, D. C.: American Council on Education, 1940. Pp. ix + 109.

Anyone who employs Thorndike or other word lists as bases for educational problems should read this monograph to see just what such lists do and do not represent. This excellent monograph reviews the bases for constructing the lists, their nature and interrelations, and their educational applications and limitations.

A problem so apparently simple as the definition of "a word," the unit of measurement, raises the question of whether we mean "a dictionary entry" (either a root or derivative) or "a semantic unit," any separate meaning of a word. Something of the magnitude of the problem is shown by the fact that the first 850 of Thorndike's "words" include an average of 24.8 separate listed meanings in the Oxford Dictionary! Ogden's "Basic English" list of 850 words based upon those logically necessary for everyday affairs contains only 14.6 meanings per word because it eliminates many unnecessary "operator" words (verbs) which have an average of about twice as many meanings as those of meanings of "general things" and "qualities." Thorndike is now compiling a vast Semantic Count which will distinguish homographs (e.g. bear, an animal, and bear, to carry) as well as separate meanings and still give indices for frequency and range of usage.

Practically all word lists suffer from inadequate attention to the sources of materials sampled, which results in relatively wide variations in credit numbers assigned above the first one or two thousand words. Earlier lists took account of frequency of use alone, but since Thorndike's later work many have followed him in taking account also of the range or variety of materials in which a word occurs. C. K. Ogden, in his Basic English, has attempted to select 850 general English words which would suffice for the handling of everyday affairs and which, with an additional 150 technical words for any given profession, would do for international correspondence. With this brief list and a limited number of grammatical forms a foreigner could thus correspond with about half the people of the earth, all those who now understand English. His list of words most necessary (logically) in most communications corresponds only partially to Thorndike's list of 1000 most commonly used words, but the two lists are also combined in a third, that of the joint Interim Committee.

It should be specifically noted that there is only a moderate correlation between frequency of use and difficulty of understanding. This is due in part to the fact that most lists are based on *adult* usage, whereas the applications are to the order of difficulty for learning by *children*. A few lists have studied the order of appearance of words by children's ages and grades.

Many educators are familiar with such statements as those of Eldridge that the first 750 words of his list of 6000 "constitute more than three fourths of all the words on the eight pages (newspaper) from which they have been drawn," and that anyone knowing these 750 words would therefore know three words out of every four they read. For a somewhat different sample containing principally personal and business correspondence, Ayres found that 300 words included 75% of all words encountered, and the commonest 1000 words included 90% of all in the sample.

However, a little further analysis shows that in another count 42

words accounted for one-half of a sample reading, but these 42 words were largely functional expressions of grammatical *relationships*, while the main *meanings* came from the somewhat rarer words such as nouns. The great numerical influence of these few common connectors at least partially distorts our conceptions of the importance of the rarer words which are essential to understanding even if they occur in fewer instances.

One experiment of Nolte (p. 56) suggests that we have perhaps overestimated the importance of limiting vocabularies in teaching children. Translating literary works into Ogden's Basic English of 850 words with simplified grammar or Thorndike's 2500 most common words added little to children's comprehension of the material. Maybe children don't need such simplified vocabularies as we have thought! Unfortunately for the purposes of integrating our experimental findings the monograph was evidently in press before the recent publication of Seashore and Eckerson (*J. educ. Psychol.*, 1940, 31, 14-38). This study shows that practically all of the earlier estimates of total individual vocabularies are serious underestimations because they failed to give persons an opportunity to show all of the words they knew, through failure to use *unabridged* dictionaries as a basis for sampling word knowledge.

It would be an injustice not to point out the value of word lists for the learning of foreign languages, for which there are various special word lists in each of the major modern languages, and another list giving the words most commonly used by all of the modern languages. Similarly, the restricted lists for teaching blind and deaf children should be of great value, while other lists have been of value in constructing systems of shorthand, the first, incidentally, being essentially a common word list (nonphonetic) published in 1588!

In spite of these specialized uses and the value of considering the *order* in which words may be most efficiently taught, the reviewer would like to add one finding of his collaborator, Dr. Mary Katherine Smith, who has just shown that in an oral sampling of the unabridged dictionary the average first-grade child can recognize a meaning of about 16,000 basic words (e.g. loyal) and 7000 derivatives, and that by the twelfth grade the figures are, respectively, 47,000 and 33,000 terms. Seashore and Eckerson have also found corresponding averages of 61,000 and 96,000 for university graduates. Users of Thorndike and other such word lists might well reconsider their estimates of alleged vocabulary limitations of children and adults and consider also the rarer words omitted by word counts but used in everyday life by even children.

ROBERT H. SEASHORE,

Northwestern University.

LANDIS, C., & coauthors. Sex in development: a study of the growth and development of the emotional and sexual aspects of personality together with physiological, anatomical, and medical information on a group of 153 normal women and 142 female psychiatric patients. (With a Foreword by N. D. C. Lewis.) New York: Hoeber, 1940. Pp. xx+329.

This monograph is a factual, rather than an interpretative, report of

the findings from a study of the sex life of 295 women, of whom 142 were patients in a hospital for mental diseases and the remaining 153 were "normal," at least in the sense that they had never been institutionalized. The authors are careful to point out that both groups are highly selected. The hospital patients, with a few exceptions, were obtained from the New York Psychiatric Institute, which accepts only cases that are of interest for research, therapy, or teaching purposes. The subjects used in this study included only cases whose disorders were without known organic bases and who were mentally capable of responding to questions in a rather long and detailed interview and of taking such tests as the Strong Vocational Interest Questionnaire. The normal subjects were volunteer participants obtained through the cooperation of various women's organizations. The groups were roughly similar as to age, social and educational background, race, and marital status. In both groups the age range was restricted to the period from 15 to 30 years for single women and from 22 to 35 for the married. The selection of relatively young subjects was decided upon because much of the information requested had to do with experiences of childhood and adolescence, and it was thought that errors of memory would be less frequent if the events described were not too remote in time.

The authors state that "the essential object of this study revolved around the following three questions: What is the normal (average) pattern of psychosexual development? How do deviations in this pattern affect the adult personality? What were the characteristics of psychosexual development of different types of adult personalities?"

Before passing on to a consideration of the findings, it may be well to consider the manner in which the investigation was set up, with reference to the three objectives specified. Clearly, we are on hazardous grounds in assuming that a group of subjects secured as these were can provide much in the way of truly normative data on anything but the most crude aspects of development in any specified area. True, they can tell something of their own experiences, but in the absence of all except the most elementary data about home and family background one can barely hazard a guess as to the character of the population for which the facts reported can be assumed to be typical. Were this a pioneer study, the long tables of percentages given in the appendix and the running summaries of these percentages that make up the body of the report might still have some normative interest, just as the early standards for intelligence tests that were generally derived from hugely biased samples nevertheless made a substantial contribution to our knowledge of the usual course of intellectual development at a time when ignorance was so great that almost any datum based upon objective study was likely to be more nearly correct than unverified guess. Even in the field of sex behavior, however, these pioneering days are over. For material of this kind to have normative value, the question of sampling must be given far more careful attention than was done in this instance. Before leaving this point, it is pertinent to wonder whether the mere fact of volunteering or not volunteering to participate in a study so highly charged with emotional determinants as this one may not be in itself one of the most important criteria available

for separating groups of unlike attitudes and experiences. Until further information is available, we should be wary in assuming that the reported frequencies of various aspects of sex experience and behavior are similar to those of women in general.

The suitability of the method for obtaining an answer to the second question is also doubtful. Inasmuch as the data are based almost wholly upon a systematic interview, with no possible means of checking on the factual accuracy of the information given, the interpretation of a stated difference in the early history of well-adjusted and poorly adjusted individuals is not easy. Perhaps it would be safer to phrase the author's second question as follows: How do deviations in adult personality affect their reports of childhood psychosexual experience? I agree with the authors that whether factually true or not, these reports are significant as personality indices at the time of reporting, but that is not the point at issue.

The third question, as here treated, may be looked upon as a specialized aspect of the second, but it is approached in a somewhat more direct fashion, inasmuch as the division into types (normal and institutionalized abnormal) is objective and the differences in the reports of childhood experiences and adult attitudes given by the two groups have present interest regardless of their historical accuracy. Moreover, this is a field that has previously received little systematic study. The finding that the mentally abnormal group reported early conditions less favorable to healthy sexual attitudes is in line with expectation, but it must not be forgotten that the present mental condition of the respondents may have colored the memory of these early events to an unknown extent. For all items reported there was marked overlapping between the normal and the abnormal group and between those members of the normal group who were judged to have good sexual adjustment and those classed as sexually maladjusted.

Perhaps the most significant feature of the book is to be found in the report of significant correlation between certain bodily characteristics, especially the form of the pelvis and measurements of the external genitalia, and sexual adjustment. Each member of both groups was given an unusually complete physical examination with special attention to the genital organs and to secondary sex characteristics. It was found that sexual response was reported as adequate by a significantly greater percentage of the normal married women for whom the clitoris-meatus distance was less than 3.5 cm. than by those for whom it was greater than this amount. Also, the normal women with the conventional feminine (gynecoid) pelvic proportions showed significantly better psychosexual adjustment than did those with pelvis showing marked narrowing of the transverse diameters, resembling that of the anthropoid apes. The fact that neither of these relationships held good for the abnormal group and that there was no apparent difference between the normal and the abnormal group in the distribution of these characteristics suggests that further verification of the finding is needed.

A considerable amount of data is presented on such matters as amount and source of early sex information, masturbation, "dates" and "crushes," parental discipline and parental attitudes, and the like, as well as upon

adult sex practices. This adds little to the information already existing on these subjects that has been obtained by similar means. The four appendices include a complete set of interview forms and other records used, copies of the "evaluation forms" used in classifying various aspects of psychosexual adjustment, and statistical tables showing the relative frequency of various reported conditions according to the marital status and mental condition of the subjects and the interrelationships among various psychosexual types.

FLORENCE L. GOODENOUGH.

University of Minnesota.

BARTLETT, F. C. *Political propaganda*. Cambridge, Eng.: Univ. Press, 1940. Pp. x+158.

The writing of this tiny book was completed early in 1940. The date is important, as much that has happened in Europe since then has been blamed, rightly or wrongly, on propaganda. The writer is a British experimental psychologist who for some time has been interested in such social matters as rumor spread. The fact that the author is British is also important, for who, in the throes of a bitter war, can treat of his enemies' achievements with proper scientific objectivity? Nowhere can Bartlett see the slightest success attributable to German propaganda techniques. He is less blinded where Russia is concerned and becomes almost patronizingly friendly regarding the Italian attempts at persuasion.

Few authorities agree on their definitions of propaganda. For Bartlett it is the "attempt to influence opinion and conduct—especially social opinion and conduct—in such a manner that the persons who adopt the opinions and behaviour indicated do so without themselves making any definite search for reasons." Education differs in that "the persons who think and act are stimulated to seek to understand for themselves why they do what they do" (p. 6). In the sections in which Bartlett discusses totalitarianism this distinction remains reasonably clear. But when he touches upon the ideals of a democratic propaganda the difference between propaganda and education becomes microscopic. Perhaps this difficulty is merely a reflection of the obvious fact that persuasive measures must vary from ideology to ideology.

Several chapters of the book are devoted to the growth, methods, and effects of political propaganda. Here Bartlett admittedly borrows heavily from *Propaganda and dictatorship*, edited by H. L. Childs, and *Public opinion*, by William Albigh. The most important chapter for Americans is undoubtedly the last, in which the author's idea of proper 'propaganda for democracy' is presented. Quite sensibly, it seems to the reviewer, Bartlett concludes that totalitarian propaganda methods are not adapted to democracies; otherwise they just could not be democracies. Yet even in England censorship is found to be necessary.

Political propaganda offers little with which the American student of propaganda is not already acquainted. Nevertheless, as it presents what is probably the approved British view, the reviewer, for one, did not consider the time devoted to the reading of it wasted.

P. R. FARNSWORTH.

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LAZARSFELD, P. F. *Radio and the printed page: an introduction to the study of radio and its role in the communication of ideas.* New York: Duell, Sloan, & Pearce, 1940. Pp. xxii + 354.

With the publication in 1935 of Cantril and Allport's *The psychology of radio* it seemed as if psychologists had finally become aware of the tremendous influence of radio broadcasting on human behavior. The man on the street had been aware of it for at least a decade. Yet the index numbers of the *Psychological Abstracts* for the years 1936 to 1940, inclusive, list under "Radio" the following number of abstracts for the successive years: 9, 4, 23, 23, 14—a total of 73 abstracts for the five-year period. During the same years the number of abstracts listed under "Rat" were: 94, 81, 77, 60, 83—a total of 395. Do psychologists really believe that rats are more than five times as significant as radio?

Psychologists should feel greatly indebted to Lazarsfeld. In the first place, *Radio and the printed page* makes available the results of much of the research that has been done on radio and the radio audience. It summarizes many of the studies made by the Office of Radio Research of Columbia University (formerly of Princeton University). Since techniques had to be devised and refined as the studies proceeded, and since field studies from which most of the data were derived are expensive, the reader should not be disappointed by the unevenness of treatment accorded the different topics.

The first chapter shows the cultural stratification of listeners to different types of radio programs. It refutes the idea that serious broadcasts reach levels of the population not reached by serious books and magazines. In the second chapter are presented the elements which make programs appealing. Effective methods of building audiences for serious programs are described in Chapter III; it should be read by every educator and educational psychologist. The fourth chapter suggests a plethora of specific problems for psychological research on the factors which distinguish people who read from people who listen. The fifth and sixth chapters examine the influence of radio upon the reading of newspapers and books.

The book describes several new techniques that should enrich social psychology. Some psychologists will be irritated by the apparent lack of statistical rigor, by the failure to mention standard errors or coefficients of correlation. Their irritation may stimulate them to attempt to refine the techniques; or it may lead to a skepticism of the elaborate statistical treatment so frequently accorded trivial data. Either result, in the reviewer's opinion, will be all to the good.

Some will be irritated by the carelessness and journalistic character of much of the writing. There are numerous typographical errors and a few quaint blunders. "The following question was asked: 'Did the radio news broadcasts increase or decrease your interest in newspaper stories?' . . . 79.4 per cent answered, 'Yes' " (pp. 260-261). The author wrote hurriedly. He wrote with a feeling of urgency that more psychological writers might feel if they were convinced of the significance and vitality of their writing.

Lazarsfeld is not content with intoning: "These data show . . ." His book is dominated by a positive social viewpoint. "By the grace of

history," he says in the introduction, "this country has been left time to solve some of the problems which have precipitated chaos in Europe. We ought to use this time to understand what social forces are operating and to adapt our thinking and our way of life to a greatly changed situation" (pp. xvii-xviii).

Psychologists will realize their indebtedness to the author only if they read his book. The reviewer highly recommends that they do read it. It will be engrossing not only to "social psychologists," but also to those psychologists who chance to be interested in human beings and the things that make them behave the way they do.

PAUL J. FAY.

DePauw University.

MACCRONE, I. D. *Race attitudes in South Africa: historical, experimental and psychological studies.* London: Oxford Univ. Press, 1937. Pp. xiv + 328.

In the book under review the author, professor of psychology at the University of Witwatersrand, Johannesburg, has attempted to measure the attitudes of various groups towards the natives of South Africa. From a perusal of the historical introduction one is inclined to the view that in few places is the opportunity so rich or the need so great for studies of the present type and value.

The author has conveniently divided his book into three sections: historical, experimental, and psychological. In order to give the necessary background for an understanding of this specific racial problem he has commenced his history with the impact of the Europeans on the natives during the Fifteenth and Sixteenth Centuries. With analytical skill he carries on his interesting narrative through the development of the Dutch East India Company under its various leaders, European contact with the indigenous population, wars against Hottentots and Bushmen, raids by Kaffirs, and the perils which beset the frontier society up to the Great Trek of 1835.

While a social attitude is always the attitude of some individual, it represents the effect produced in him by his social environment. An attitude is at one and the same time "a determining tendency, a mode of adjustment and a preparation for response to specific situations" (p. 143). Opinions may be regarded as verbalized attitudes, and these, with overt acts, enable us under certain circumstances to form judgments concerning the attitudes of the individual.

In his experimental work, the author has developed a scale for measuring attitudes towards natives. A large number of opinions representative of current attitudes were taken and by valid procedure reduced to thirty items. The reliability of the scale as measured on a group of more than 200 university students was .90 (split-half method) and .935 (test-retest method). Evidence is presented for the validity of the scale.

A further measure of attitude was attempted by the construction of a questionnaire for the purpose of measuring social distance. Omitting one special group for adequate reasons the reliability of the questionnaire varied from .64 to .91. Validity of the questionnaire seems to be indicated

by its apparent correspondence with the prevalent attitude of certain criterion groups towards natives, *e.g.* the Afrikaans-speaking group is generally regarded as being the group least favorable in its attitude towards natives and measures thus on the questionnaire.

The scale and the questionnaire in their final forms were applied to more than 600 students of both sexes in universities and colleges in South Africa.

Applying Spearman's method of tetrad differences, the author sets out to determine the possibility of common factors in the attitudes exhibited by racial groups. His conclusion is that there are factors which are common to a number of groups. Of these common factors "the two most important are (a) the factor which is common in attitude towards the native and social distance to correlated groups, and (b) the factor which is common to social distance to the in-group and social distance to correlated groups" (p. 230).

In his concluding section the author describes the psychosocial factors which he believes responsible for the development of attitudes and social distances. From a criticism of McDougall's "instinct hypothesis" he passes to an acceptance of Freudian psychology in his analysis of causes. Through Freudian group psychology to the mechanics of identification we are led to believe that prejudice against the out-group is to be regarded as "increased intensity of identification between members of the in-group, since a common object of hate is, in some respects, a stronger bond between individuals than a common object of love" (p. 253). For this reason, he argues, the antagonistic attitude towards the native may serve its fundamental purpose—the preservation of the white group.

A third study sought to discover the factors affecting the white attitude through a "free" questionnaire containing thirty items. The replies to the questionnaire and numerous examples of current history form the basis of the argument in the last chapter—mainly psychoanalytical.

Thus we find displayed in the social attitudes of the members of the white group towards natives those very features of aggression and repression, of segregation and isolation, of projection and phobia formation, which represent so many duplications of the defence mechanisms of the neurotic individual. The extra-individual conflicts between the racial groups are but the intra-individual conflicts within the mind writ large, and until the latter are removed, reduced or modified they must continue to exercise their baleful influence upon the race relations and the race contacts of white and black (p. 310).

While there are some who will hesitate to accept the Freudian analysis put forward by the author, there is little doubt that in his experimental studies he has made a distinct contribution to race psychology. His rigidly applied experimental technique and his clear perception of the essential elements of the problem make the book an interesting and valuable contribution to racial and social psychology.

CECIL W. MANN.

University of Denver.

GLOVER, E. (Ed.), with the assistance of M. Brierley. The investigation of the technique of psycho-analysis. Baltimore: Williams & Wilkins, 1940. Pp. ix + 188.

This book reports the results of an investigation bearing on the empirical techniques of psychoanalysis. A questionnaire was sent to twenty-nine British analysts, and answers were obtained from twenty-four. The questions embrace the whole field of practice and include such problems as interpretation, "active" devices, termination of analysis, the neutrality of the analyst, fees, extracurricular contact, relation of theory to practice, and the handling of psychotic cases.

The results, which are discussed at length in the different chapters, are assembled under four summary headings. In the first of these, *A*, are included points on which there is (almost) complete agreement. In this category we find a statement of "universally accepted tenets." These include such practices as, for example, analyzing questions rather than answering them, analyzing the transference, and avoidance of social contact during treatment. Under *B* are included general tendencies of practice which are supported by at least two-thirds of the contributors. To judge by the result, most of the practitioners favor such diverse procedures as rapid interpretations in acute anxiety, treatment of "common-sense" reactions as reality defense, emphasis on the importance of childhood memories, and permission for the patient to smoke.

Points on which there is a marked division of opinion fall under *C*. Most striking of the problems which fall in this category is the relation of theory and practice.

Several analysts felt unable to reply, or replied only in part, to this question. The rest appeared to be equally divided. Thus, one-half denied that they approach analysis with a preconceived outline in mind. Half the remainder admit to a very general outline, partly theoretical, partly modified by diagnosis and prognostic expectations. A few admit outright to a "modified" outline, and two more admit such an outline but try to minimize its importance.

Individual opinions, strongly expressed, are considered in a final section, *D*. Here are collected a sample of "pet" beliefs and practices of individual analysts. At least one believes that women analysts are better for both sexes, and another decries systematic interpretation.

The foregoing examples are only slightly indicative of the minutiae of the analyst's stock in trade which have been subjected to inventory. Because Glover writes about practical aspects rather than theory, his book may be illuminating to psychologists whose experiences with psychoanalysis are largely academic. In any event, "The Technique of Psychoanalysis" gives an account of what psychoanalysts do rather than what they think. For this reason it seems to be a first step along the path of operational specification.

C. H. GRAHAM.

Brown University.

MARZOLF, S. S. *Studying the individual: a manual on the case study for guidance workers and psycho-clinicians.* Minneapolis: Burgess, 1940. Pp. vii + 181.

The author says (p. i): "Considerable effort has been made to make the student aware of the niceties of the experimental method. . . . However, surprisingly little has been done in this respect for the case method. Such is the aim of the problems here presented." The problems—eighty-four of them—include such questions as: "*Problem 14.* How does the hypothesis influence the course of the inquiry?"; "*Problem 59.* Does the response to treatment prove the correctness of diagnosis?"; and "*Problem 83.* In what fields, and with what kinds of problems, may the case method be used for inductive purposes?" The plan of the book is to follow the citation of each problem with a brief portion of some case record, then to propose provocative questions and exercises relating either to the case or to the general problem, and finally to present a brief discussion of the problem together with annotated references. The author has in mind the use of the book by seniors and first-year graduate students who may be participating in a seminar or practicum in the making of case studies. Some knowledge of case problems and abnormal psychology is presupposed.

There is no doubt that the author has done a lot of issue raising. One is reminded of graduate examining committees, where there is certain to be at least one member who feels moved to ask the candidate something academic and controversial, but who, because he lacks a fine sense of live and dead issues, succeeds in becoming more exasperating than stimulating. The reader who intends to follow out each of the suggested exercises should first trim his fingernails very short.

The text itself contains very little new information for those who are already well enough informed to follow its discussions. The references, however, seem to be well selected and more than ample.

The excerpts from case records or, in some instances, the narratives of professional situations which accompany each problem make interesting reading and lead one to expect a vivid concrete discussion of the problem. Apparently the author originally planned that they should lead into such discussions. In fact, however, most of the discussions are of a very abstract nature, and many of the case excerpts bear only the most remote applications of the problem at hand.

The author believes in the case method. He believes a better understanding of it and a more rigorous application of its principles will raise it to the highest level of respectability. That faith is held in common with an increasing number of his colleagues who could wish for him that his original idea for making his discussions more concrete had been followed more effectively, that his issues had been more lively, and that he will continue his contributions to the case method.

GEO. A. KELLY.

Fort Hays Kansas State College.

JERSILD, A. T. *Child psychology.* (Rev. & enlarged.) New York: Prentice-Hall, 1940. Pp. xiii + 592.

This book is an extensive revision and enlargement of the author's well-known first edition. A comparison of the two editions reveals noteworthy changes in viewpoint, contents, and mechanics.

Those who object to explaining psychological behavior in terms of internal mechanisms may feel disappointed when they read on page 3 that "the higher brain centers . . . are man's main possession and pride" lest this presages the "certain changes in viewpoint and emphasis" mentioned in the preface. But such persons will encounter only one or two neurological explanations of behavior in this book. Rather, their primary concern will be for the author's abandonment of the viewpoint of the first edition that physiological maturation provides certain potentialities and limitations for behavior development, to flirt with the notion of the priority of "nature over nurture." Throughout the book the gratuitous question is raised whether various types of behavior are innate or acquired, e.g. parental sentiments (p. 156), social behavior (pp. 166-167), cooperation (p. 200), facial expressions of emotions (p. 247), fear (p. 256), anger (p. 284), affection (p. 308), sympathy (p. 315), crying (p. 317), moral conduct (p. 400), honesty (p. 409), sociability (p. 434), intelligence (pp. 491 and 521), personality (p. 535), etc. The most direct revelation of the author's change in viewpoint appears on page 309 in a footnote reproduced here as follows:

The term *instinct*, which flourished in psychological writings some years ago, has gone out of vogue, largely because forms of behavior that once were regarded as instinctive or inborn have been found to be influenced considerably by acquired habits. The term has not been used much in this book, not because of any particular prejudice against it, but partly for the sake of using terms more commonly accepted at the present time and partly because of the distorted meanings attached to the term in popular usage. It is quite possible that psychology is reaching the end of the anti-instinct cycle, however, and that the basic concept denoted by the term will be revived, in a modified form and probably under a different name.

Many psychologists familiar with the earlier edition may feel that the author's change in viewpoint is something of an intellectual regression, especially in the light of the widespread concern in psychology within the past several years for problems of scientific methodology and postulation. If the viewpoint of the revised edition could be isolated as such, it might be more easily disregarded, but no statement of it appears in any one place. Rather, it is expressed largely as a matter of discussion emphasis and in the choice and interpretation of research literature.

However, psychologists who are not too critical of the author's change in viewpoint and who are looking for a text with plenty of undergraduate appeal will do well to consider this book. It is, if anything, even more readable and interestingly written than the earlier edition.

In content, the revision includes new chapters on learning and growth, routine physical habits in early childhood, children's interests and activities, and children's moral and religious concepts. The three chapters treating emotions have been expanded considerably, and the material on social behavior substantially recast and reorganized into two chapters, "in keeping with the importance of this aspect of development from the point of

view of the welfare of the individual and the light which it throws upon adult affairs." All other chapters have been appreciably revised to include accounts of more recent research literature. The treatment in general emphasizes child behavior in school situations more than do most books in this field. For this reason, the book should be of special interest to students of education, but by no means is its appeal limited to this class of students.

Much larger bibliographies appear at the end of each chapter, and bibliographic references by name and/or number are now interspersed throughout the text. The volume is even better indexed as to subject and author, its page size is larger, and the typography improved.

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NOTES AND NEWS

AT the meeting of the Society of Experimental Psychologists at Rutgers University, March 27 and 28, the Warren Medal was awarded to Clarence H. Graham for his determination of the area-intensity function in the sensitivity of the human retina.

DR. RICHARD W. HUSBAND, assistant professor of psychology at the University of Wisconsin, has recently been appointed visiting assistant professor of psychology in Extension at Pennsylvania State College. He is associated with the Extension Services in the foreman training courses of the Engineering Defense Program. Dr. Husband's special field is that of business and industrial psychology.

PROFESSOR J. U. YARBOROUGH, head of the department of psychology at Southern Methodist University, has been granted a leave of absence to serve as administrative supervisor of the Merit Council of the Unemployment Compensation Commission of Texas. This Council will have charge of setting up a civil service system for certain departments of the state government. Mr. William H. Lichte, Ph.D. from the University of Iowa, has been appointed instructor in psychology, and Professor A. Q. Sartain has been made acting head of the department of psychology.

DR. HULSEY CASON, who since 1940 has been a psychologist in the United States Public Health Service, is at present engaged in full-time experimental and physiological research at the Medical Center for Federal Prisoners, Springfield, Missouri.

THE recently formed Canadian Psychological Association has begun the publication of a small bulletin, under the editorship of D. O. Hebb, Queen's University, Kingston, Ontario, with the purpose of keeping its rather widely scattered members in touch with one another's work. The *Bulletin of the Canadian Psychological Association* will, for the present at least, accept only brief preliminary abstracts of research normally to be published in full elsewhere. The subscription is \$1.00 per year.

A research grant of \$900 has been awarded by the Research Corporation of New York to the University of Pittsburgh for support of continued research on the relation between the nutritional state of experimental animals and their resistance to pathological disturbances in behavior. The research is being carried on in the laboratories of experimental psychology by Mr. Robert A. Patton under the direction of Dr. Harry W. Karn. Dr. C. G. King, of the department of chemistry, is advising on the nutritional aspects of the investigation. Preliminary work on the problem was made possible by a grant from the Buhl Foundation of Pittsburgh.

THE *Directory of Applied Psychologists* has been published by the American Association for Applied Psychology. A limited number of copies are available at \$1.00 postpaid.

WORD has been received by Leonard Carmichael, president of Tufts College, that a new "China Institute of Physiology and Psychology" has been established at Chungking. The well-known Chinese psychologist and experimental embryologist, Dr. Zing Yang Kuo, has been named director of the Institute.

DR. S. J. BECK, head of the psychology laboratory in the department of neuropsychiatry at Michael Reese Hospital, will offer his usual summer course on "The Rorschach Test in Personality and Clinical Diagnosis" from June 23 through June 27, 1941. The course teaches the technique of administering the Rorschach Test and scoring the responses. It orients the student into interpretation with special emphasis on clinical classification. Response records as obtained from various healthy personality groups and from clinical groups (schizophrenia and some neuroses to be included) will be scored, analyzed, and interpreted. The primary aim of the course will be to demonstrate the test's practical application in investigating the whole personality, with particular reference to its clinical use. Those interested may communicate with the Medical Librarian, Michael Reese Hospital, 2908 Ellis Avenue, Chicago, Illinois.

Two Workshops in the Rorschach Method will be conducted during the Summer Session at Western Reserve University by Dr. Marguerite Hertz, President of the Rorschach Institute. The first, "Introduction to the Rorschach Method of Personality Study and Clinical Diagnosis," will be held from June 23 to July 11; the second, "Advanced Course in the Clinical Application and Interpretation of the Rorschach Method," from July 14 to August 1. Additional information may be obtained from the Secretary, Department of Psychology, Western Reserve University, Cleveland, Ohio.

Sociometry announces that Dr. Gardner Murphy has resigned as editor of that journal and that his place has been taken by Professor George A. Lundberg. Dr. Murphy remains as a member of the Editorial Board. Dr. Barbara S. Burks has joined the contributing editors. Information concerning the 1941 Summer School of the Psychodramatic Institute may be obtained from the Institute, 259 Wolcott Avenue, Beacon, New York.

THE Surgeon General of the Navy invites the attention of psychologists to the opportunity of becoming commissioned officers in the Volunteer, Special Service, Class H of the U. S. Naval Reserve.

Male citizens of the United States who are physically and otherwise qualified are eligible for appointment as commissioned officers in class H-V(S) of the U. S. Naval Reserve.

A candidate for appointment in class H-V(S), U. S. Naval Reserve, must present the following credentials as to education and professional qualifications:

(a) Certificate of, or evidence of, at least four (4) years of collegiate education; or a degree from an accredited institution of higher education in a subject or subjects which pertain to, or are related to, those specialties

coming under the cognizance of the Medical Department of the Navy, the determination of which shall be made by the Chief of the Bureau of Medicine and Surgery.

(b) Evidence of license to practice their profession in a state or territorial possession of the United States where such is required.

(c) If the candidate has had special training, a certificate to this effect shall be included.

(d) Evidence of qualification in specialty, which shall be satisfactory to the Chief of the Bureau of Medicine and Surgery.

(e) In addition to the requirements listed above, candidates shall submit evidence of recent practical experience in their specialties as indicated:

For Ensign (2) or more years

For Lieutenant (junior grade) (6) or more years

For Lieutenant (8) or more years

(f) The rank in which candidates for class H-V(S) are appointed will be determined by the candidate's age, academic seniority, and practical experience. These must be appropriate to the duties of a specific mobilization assignment.

In addition to the above basic requirements, applicants for appointment as psychologists should have a certificate or professional degree from an acceptable institution and submit evidence of practical experience in actual individual counselling and examining, preferably with adults, in prisons, hospitals, college guidance, or other institutions.

It is contemplated using the services of such officers in examining recruits of the Navy and Marine Corps at Naval Training Stations, Marine Corps Depots, and Aviation Procurement Centers.

Psychologists desiring appointment as commissioned officers in class H-V(S) of the Naval Reserve should communicate with the commandant of the naval district in which they reside, as listed below, requesting application forms and any further information they may desire regarding such an appointment.

Addresses of Commandants

Commandant, First Naval District, North Station Industrial Building, 150 Causeway Street, Boston, Massachusetts.

(States of Maine, Massachusetts, New Hampshire, Vermont, and Rhode Island, including Block Island, and all U. S. Naval Reservations and Naval Activities on shore in Newfoundland.)

Commandant, Third Naval District, Federal Building, 90 Church Street, New York, New York.

(States of New York, Connecticut, and upper New Jersey, including counties of Mercer, Monmouth, and all counties north thereof, also Nantucket Shoals Lightship.)

Commandant, Fourth Naval District, Navy Yard, Philadelphia, Pennsylvania.

(States of Pennsylvania, southern part of New Jersey, including counties of Burlington, Ocean, and all counties south thereof; Delaware, including Winters Quarter Shoal Light Vessel.)

Commandant, Fifth Naval District, Naval Operating Base, Norfolk, Virginia.

(States of Maryland, except Prince Georges, Montgomery, and Charles Counties; Virginia, except Arlington, Fairfax, Stafford, King George, and Prince William Counties, and the city of Alexandria; West Virginia; and the counties of Currituck, Camden, Pasquotank, Gates, Perquimans, Chowan, and Dare in North Carolina, also the Diamond Shoal Lightship, and all U. S. Naval Reservations and Naval Activities on shore in the Islands of Bermuda.)

Commandant, Sixth Naval District, Navy Yard, Charleston, South Carolina.

(States of South Carolina, Georgia, and North Carolina, except the counties of Currituck, Camden, Pasquotank, Gates, Perquimans, Chowan, and Dare.)

Commandant, Seventh Naval District, Navy Yard, Charleston, South Carolina.

(State of Florida, except counties west of Apalachicola River.)

Commandant, Eighth Naval District, New Federal Building, New Orleans, Louisiana.

(States of Alabama, Tennessee, Louisiana, Mississippi, Arkansas, Oklahoma, Texas, and Florida, except counties east of Apalachicola River.)

Commandant, Ninth Naval District, Naval Training Station, Great Lakes, Illinois.

(States of Ohio, Michigan, Kentucky, Indiana, Illinois, Wisconsin, Minnesota, Iowa, Missouri, North Dakota, South Dakota, Nebraska, and Kansas.)

Commandant, Tenth Naval District, San Juan, Puerto Rico.

(All island possessions of the United States pertaining to Puerto Rico and the Virgin Islands, and including Guantanamo, Puerto Rico, Vieques, Culebra, Virgin Islands, and recently acquired British Bases: located at—Bahamas, Jamaica, Antigua, St. Lucia, Trinidad, and British Guinana.)

Commandant, Eleventh Naval District, Naval Station, San Diego, California.

(States of New Mexico, Arizona, southern part of California, including counties of Santa Barbara, Kern, and San Bernardino, and all counties south thereof.)

Commandant, Twelfth Naval District, Federal Office Building, Civic Center, San Francisco, California.

(States of Colorado, Utah, Nevada, northern part of California, including counties of San Luis Obispo, Kings, Tulare, Inyo and all counties north thereof.)

Commandant, Thirteenth Naval District, 553 Federal Office Building, Seattle, Washington.

(States of Washington, Oregon, Idaho, Montana, Wyoming, and Territory of Alaska.)

Commandant, Fourteenth Naval District, Naval Station, Pearl Harbor, Hawaii.

(Hawaiian Islands, and islands westward, including Midway.)
Commandant, Fifteenth Naval District, Naval Station, Balboa, Canal Zone.

(Panama Canal Zone.)

Commandant, Sixteenth Naval District, Naval Station, Cavite, Philippine Islands.

(Philippine Islands.)

Commandant, Navy Yard, Washington, D. C.

(District of Columbia, Prince Georges, Montgomery, and Charles Counties, Maryland; and Arlington, Fairfax, Stafford, King George, and Prince William Counties, Virginia, and the city of Alexandria, Virginia.)

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